AN ESSAY

ON THE

MATHEMATICAL PRINCIPLES OF PHYSICS.

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AN ESSAY

ON THE

MATHEMATICAL PRINCIPLES OF PHYSICS,

WITH REFERENCE TO

THE STUDY OF PHYSICAL SCIENCE

BY

CANDIDATES FOR MATHEMATICAL HONOURS IN THE UNIVERSITY OF CAMBRIDGE.

BY THE

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Felix qui potuit rerum cognoscere causas.—VIRG.
Τὰ γὰρ ἀόρατα, τοῖς ποιήμασι νοούμενα, καθορᾶται.—Rom. I. 20.

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INTRODUCTION.

THE contents of this work are devoted almost exclusively to discussing the *principles* and the *reasoning* appropriate to the theoretical department of Natural Philosophy, and the mutual relation between this and the experimental department. The discoveries in recent times of new facts and physical laws by experimental means have been so remarkable and abundant, and have given rise to so much *speculation*, that there seemed to be reason to apprehend that the part of philosophy which is properly *theoretical* might be either set aside or wholly misunderstood. The purpose of this Essay is to endeavour to counteract this tendency.

The method of theoretical philosophy adopted in this work, as well as in my larger one on the *Principles of Mathematics and Physics*, published in 1869, is for the most part the same as that which is indicated by Newton in Book III. of the *Principia*. In both publications this method has been largely applied in the explanation of physical phenomena and laws, advantage having been taken for that purpose of the modern advances that have been made in physical determinations by experiment, and in abstract mathematics. The arguments employed in

its application (which, in order that they may be more generally understood, are exhibited in the present work with as little reference as possible to analytical formulæ) have conducted to explanations so numerous, and of such different kinds, as apparently to justify the conclusion that this System of Philosophy is true both as to its principles and as to the reasoning it demands; in short, that it is the true method of theoretical philosophy. With the view of assisting to form a judgment on its essential character, and on the necessity arising therefrom for adopting it, I propose to make in this Introduction some brief remarks on the antecedent history and actual state of the theoretical department of Physics.

All theoretical investigation is carried on by means of calculation, and the calculation is not simply algebraic, but consists essentially of the formation, according to given conditions, of *differential equations*, and the solutions of them by the rules of analysis. Newton is therefore to be regarded as the founder of theoretical philosophy, having in his *Principia*, by reasoning of which there had been no previous example, virtually formed and solved the differential equations which are necessary for calculating the motions of the Moon, the Earth, and the Planets with their satellites. And because these calculations rested on the hypothesis of an *attractive force* varying with distance according to the law of the inverse square, the motions, by being thus calculated, were referred to an operative *cause*. Kepler's laws, as determined by observation, are merely formal relations of time and space, and, like all quantitative laws, are deducible by calculation founded on the hypothesis of a producing cause, being, in fact, *problems* demanding

such a solution. Newton solved these problems by his theory of gravitating force. It is evident that a step like this, inasmuch as it establishes the reality of causative action, is an advance of physical science beyond the mere knowledge of formal laws.

The truth of the hypotheses on which the theory of universal gravitation rests is not capable of demonstration by calculation alone, nor by observation alone, but by a combination of both. Such experiments as those of Galileo for determining the laws of the vertical descent of bodies acted upon by the earth's gravity, and those which prove that the vertical acceleration of a projectile by the same force is independent of its motion in a curvilinear path and of the rate of such motion, were absolutely necessary for the discovery of the fundamental laws on which, as hypotheses, all calculation relative to the motions produced by accelerative forces must rest. These hypotheses do not admit of *à priori* demonstration except on physical principles of a higher grade than those now under consideration. (See Art. 24 of the *Essay*.)

But after admitting on *experimental* grounds the above-mentioned laws respecting the action of *constant* forces, it is provable by *abstract reasoning* that an accelerative force, whether variable or constant, estimated in a given direction, is quantitatively denoted by *the second differential coefficient* of the function which expresses the distance at any time of the accelerated particle from a fixed plane perpendicular to that direction. (The demonstration as given in pp. 109—113 of *The Principles of Mathematics and Physics* is conducted by means of Taylor's Theorem, which is legitimately employed for

this purpose on the general principle that abstract calculation is comprehensive of all concrete physical relations.) On this symbolic expression for accelerative force, which, as said above, could only be arrived at by experiment and abstract calculation combined, the whole of Physical Astronomy depends, together with every department of applied science which requires the calculation of motions produced by accelerative forces. The discovery of the calculation necessary for this purpose characterizes in a special manner the Newtonian epoch of Natural Philosophy.

Besides the above-mentioned *general* hypotheses, or foundations, of all calculation relating to accelerative forces, there are *particular* hypotheses which are distinctive of the class of questions to which the calculation is applied. Such are the hypotheses of Physical Astronomy, discussed in Art. 10 of the *Essay*, respecting the mathematical deductions from which a few historical notices, illustrative of the principles of theoretical reasoning, may here be introduced. After forming the appropriate differential equations on those hypotheses, it only remained to integrate them, and to apply the integrals, according to the abstract rules of analysis. Now it appears that Newton published only an imperfect calculation of the motion of the apse of the moon's orbit, and that his successors, after trying to complete, by means of the analytical solution of the problem, what he began, failed at first in the attempt, and were disposed to conclude that the assumed law of gravity was not exactly true. Eventually, however, it was found that the *reasoning* was at fault, and the difficulty was overcome by an extension of Newton's method of treat-

ment of the problem, conducted strictly according to the rules of successive approximations.

But it is to be observed that in order to apply that method it was necessary to get rid of certain terms in the analytical solution which increase indefinitely with the time. Now since it may be laid down as universally true that inferences strictly deduced by reasoning from premises must have some significance relative to the premises, and as these terms were so deduced, it is necessary, in order to complete the mathematical theory, to ascertain their origin and meaning, and the rationale of the process for getting rid of them. To these questions I devoted much consideration, not having met with any satisfactory answers to them, and came at length to the conclusion that *every* process by which the terms of indefinite increase are avoided introduces *arbitrarily*, without altering the number of arbitrary constants, the condition that there shall be a *mean* orbit; so that those terms prove that there may be perturbed motions which do not fulfil that condition. Accordingly the Problem of Physical Astronomy is not the general Problem of Three Bodies, but the more restricted one of disturbances producing oscillations relatively to a mean orbit. Assuming, as is allowable, that the mean orbit is an ellipse the elements of which can be calculated from observational data, it follows that the mean eccentricities of the orbits of the Moon and Planets must have inferior limits. For the mean eccentricity could not be zero unless the orbit was always an exact circle, which under the actual circumstances of the attracting bodies is impossible. This argument may be considered to be necessary for completing the deductions that can be drawn by

abstract reasoning from the hypotheses of Physical Astronomy.

The foregoing remarks may suffice to shew in what way it is necessary to combine abstract reasoning with the results of observation for the formation of a theory in any other department of Physics.

Newton attempted to solve the Problem of the *Precession of the Equinoxes;* but it is evident from the mode in which he made the attempt that he had not recognized D'Alembert's Principle. This principle is indispensable for calculating the accelerated motions of *masses*, when they cannot be treated as if they were elementary particles, and is as legitimately applied to fluid as to solid masses. In fact, it is not so much a principle as an *axiom*, which admitted of being enunciated after the discovery was made that an accelerative force is quantitatively expressed by the second differential coefficient of linear space regarded as a function of the time (see Arts. 5, 7 and 8 of the *Essay*).

All the calculations relative to accelerative forces referred to in the foregoing remarks depend on differential equations which are reducible to a single one containing only two variables. For calculating the accelerated motions of a *fluid*, differential equations are required which in no case can contain fewer than three variables; on which account the analytical reasoning is of a higher order than that of the other class of problems, and is attended in its applications with peculiar difficulties. For the science of analytical hydrodynamics we are mainly indebted to the researches of Euler, Lagrange, and Poisson. Laplace, although he surpassed his contemporaries in theoretical deductions by means of the ordinary class

of differential equations, effected but little relatively to the applications of partial differential equations. I take occasion here to recommend mathematical students not to adopt the method of treatment of hydrodynamical problems employed by Laplace, and also by some English mathematicians; which, as not introducing a symbol (called ϕ by Lagrange and Poisson) to indicate the existence and variability of "surfaces of displacement," keeps out of sight an essential principle of Hydrodynamics. (See Art. 18.)

Newton gave a solution of the problem of the *velocity of sound* on the assumption (which was legitimate) that the aerial particles vibrate according to the law expressed by an harmonic function. He obtained a value of the velocity less by one-sixth than that given by experiment; and modern analytical methods have conducted to the same result, because, in fact, they do not differ in principle from that of Newton, so far as relates to the determination of rate of propagation.

I think it unnecessary to give here the particulars of the well-known theories proposed by Laplace and Poisson to account for the excess of the experimental above the Newtonian value of the velocity of sound: it will suffice to remark that their views concur in ascribing to the agency of the heat and cold developed by the condensation and rarefaction of the aerial particles in vibration, an increment of accelerative force having a constant ratio to the force due, apart from any such agency, to the actual variation from point to point of space of the density of the fluid, its temperature being invariable.

Long since I called in question the legitimacy of

inferring the amount, or even the reality, of such *instantaneous* changes of the effective accelerative force in aerial vibrations from experiments made on air in *closed spaces.* The accession, or diminution, of the temperature of the air operated upon in these experiments is apparently to be attributed to the circumstance that the heat-radiants, or cold-radiants, generated by the sudden compressions or dilatations of the air are prevented from being propagated into surrounding space by the solid boundaries of the containing vessel; the consequence being that by successive reflections at the interior surface they are made to traverse the enclosed air repeatedly, and thus, by altering the amount of heat-waves emanating from the individual atoms (see Art. 60), produce a change of temperature. According to these views the change is wholly due to the air being enclosed. But whether or not this explanation be true, the circumstances of the compression and dilatation of air in closed vessels differ from those of the condensations and rarefactions of aerial vibrations in free space in such manner as to give no experimental evidence of the effects of the development of heat and cold by the latter.

Since, however, the determination of the velocity of sound by Newton's method is a matter of *reasoning,* it is open to enquiry whether erroneous or imperfect reasoning may not account for the discrepancy between the calculated and observed values. I propose, therefore, to give here a brief account of the argument from which I have concluded in the *Principles of Mathematics and Physics* (pp. 189—207), as well as in other publications, that the reasoning commonly employed in this investigation is faulty.

After forming an exact differential equation applicable to motion of an elastic fluid which at any given time is a function of the distance from a fixed plane to which its directions are perpendicular, a particular integral is obtainable which exactly satisfies the equation (*ib.* p. 194). By giving to the arbitrary function the form of an harmonic function, whereby the differential equation is still satisfied, it may be proved by strict mathematical reasoning that "at the same distance from the fixed plane and at the same moment the velocity of the fluid may be zero and yet have its maximum value" (*ib.* p. 195). Such a result is a contradiction *per se*, indicating, according to a known rule of logic, either fault or defect in the premises, or fault in the reasoning. No fault in the reasoning being discoverable, it follows of necessity that the premises of Hydrodynamics require to be rectified. Although my mathematical contemporaries do not deny the reality of the above contradiction, they have hitherto refused to admit the necessity of the logical consequence. It is by such disregard of the rules of reasoning that error is introduced and perpetuated.

The way in which I have proposed to rectify the principles of Hydrodynamics is explained in Arts. 18 and 19 of the *Essay*. I have maintained that in addition to the two general hydrodynamical equations depending on the principle of constancy of mass and D'Alembert's principle, a *third* is necessary for expressing analytically the condition that "surfaces of displacement" may be drawn through all the elements at all times. This equation gives expression to a principle of geometrical continuity to which the motion, if it admits of being calculated, must be subject. It might reasonably be urged

that an additional general equation must modify essentially the whole of the reasoning in analytical Hydrodynamics, and the modes of its applications. That this is actually the case will appear from the following statement of results for obtaining which that equation is indispensable.

(1) It is proved independently of particular conditions as to the manner of putting an elastic fluid in motion, that *vibratory* motions, expressed by harmonic functions, take place simultaneously in directions parallel and transverse to an *axis*, such motion being due exclusively to the elasticity and inertia of the fluid. On this result the *Undulatory Theory of Light*, including the theory of polarization, wholly depends. (See Arts. 40—44.)

(2) The velocity of sound obtained by means of the same result is found to differ very little from the observed value. (*Principles of Mathematics*, &c. pp. 214—224.)

(3) When the analysis which gives the above-mentioned *spontaneous* vibratory motions is extended to terms of the second order, it can be proved that undulations incident on small spheres not only cause them to vibrate, but produce also accelerated motions of translation either in the direction of the propagation of the undulations or in the opposite direction. The translatory motion is attributable to the distribution of condensation about the surface of the sphere, as modified by the *inertia* of the vibrating fluid; on which account it is necessary to proceed to second-order terms. On this deduction from the reformed principles of Hydrodynamics *the theory of attractive and repulsive forces* depends. (Arts. 52—56.)

(4) By means of the third general equation it may be proved that *steady* streams can flow along, and in the

vicinity of, an axis of any curved form and any length, if the courses be in *spirals* about the axis, and if they complete a circuit. (Arts. 151 and 154.) This result of the analytical reasoning is the foundation of the hydrodynamical theory of *galvanism*. The phenomena of *magnetism* are also referable to steady motions of the æther, but the courses of magnetic streams are not subject to the condition of being spiral.

(5) When the motion of an elastic fluid is central, and a function of the distance from the centre, analysis shews that the condensation varies inversely as the distance; whereas if the propagation of a solitary wave of condensation from a centre be possible (as has been assumed), the condensation must vary inversely as the square of the distance. This difficulty I pointed out long ago, although I have only recently discovered the solution of it. By taking into account the *spontaneous* vibratory motions deduced, as stated above, from the rectified principles of Hydrodynamics, it appears that the generation of a solitary wave of condensation or rarefaction is not possible, inasmuch as, by reason of the elasticity and inertia of the fluid, an impulse not vibratory instantaneously excites in it alternations of condensation and rarefaction (Art. 41). [This point is discussed at the end of an article on "Attraction by vibrations of the Air" in the *Philosophical Magazine* for April 1871.]

The foregoing statement of the results deducible by means of the third general equation may be considered to demonstrate the necessity there was, as regards the advancement of theoretical physics, for thus completing the principles of hydrodynamics. The proof of the new equation is, in fact, an essential part of the analytical

reasoning by which the motion of an element which moves in juxtaposition with other elements is to be calculated. Newton discovered the mathematical reasoning proper for calculating the accelerated motion of a *single* particle: the existing state of theoretical physics demanded the discovery of the reasoning proper for calculating the motions of a *congeries* of particles constituting a *fluid*. I claim to have taken a necessary step towards meeting this demand, by completing the mathematical principles of Hydrodynamics.

Having by the preceding considerations indicated the character of the addition to the received mathematical principles of theoretical science which is proposed and maintained in the *Essay*, and having shewn by historical notices that such addition is consistent with, and was demanded by, the antecedents of experimental and theoretical physics, I have fulfilled the main purpose of this Introduction. There are, however, some remaining particulars which appear to me to call for remark or explanation.

It will, perhaps, be noticed that in this Introduction, and throughout the *Essay*, I have made very little reference to the productions of contemporary mathematicians who have written on the same subjects. The reason for this may be readily given. Although these subjects have engaged the attention of many physicists, I can point to no one who has treated of them *theoretically*, in the sense in which I understand theory. What I mean by this assertion will be perceived at once by referring to the views expressed in Art. 113 respecting Gauss's supposed proof of the law of the inverse square in magnetic action. The proof rests on hypotheses

which, as not being intelligible from sensation and experience, do not conform to the Newtonian Rule of Philosophy. I cannot see that any knowledge is gained by conclusions drawn from hypotheses that are themselves unexplainable, or unintelligible. Such hypotheses Newton referred to by saying "somnia confingenda non sunt," and again, towards the end of the *Principia*, "hypotheses non fingo." These expressions refer to *arbitrary*, as distinguished from *necessary*, hypotheses, inasmuch as the first occurs under the very Rule of Philosophy which contains, respecting the qualities of the ultimate constituents of bodies, hypotheses which Newton pronounces to be "the foundation of all philosophy." (See Arts. 26 and 27.)

Not having felt any difficulty as to the admissibility of these hypotheses, and those relating to the existence and qualities of the æther, as foundations of reasoning whereby their physical reality might either be disproved or established, I have devoted much time and labour to physical researches conducted by mathematical reasoning resting on this basis, and I cannot in the least degree understand why there should exist—as there does exist among physicists of the present day—an antecedent objection to entertaining this method of philosophy. Prejudgment is allowed to stand in the way of giving consideration to the principles of a philosophy which commended itself to the minds of Newton and Locke, and consequently explanations of phenomena mathematically deduced from them are disregarded.

Empirical formulæ derived from a large number of experimental data, such as the formulæ by which Gauss succeeded in approximately expressing certain of the laws

of terrestrial magnetism, are sometimes improperly spoken of as if they were theoretical. They are important as embracing in one view facts of the same kind, and indicating relations of groups of facts; also they may assist in forming a theory which is truly such; but they differ altogether from inferences deduced by mathematical reasoning from *à priori* principles. The distinction will be at once perceived by the contrast between Gauss's theory of terrestrial magnetism and that proposed in Arts. 120—149 of the *Essay*.

Neither can Fourier's analytical theory of heat be called theory in the proper sense of the word. For it consists of deductions, by appropriate analysis, of formulæ expressing *laws* of heat, from certain experimental data; but professedly contains nothing respecting the intrinsic nature of heat. The Theory of Heat expounded in Arts. 47—62 of the *Essay* will be seen to be something very different as respects both principles and reasoning. The value of Fourier's work consists in its grouping phenomena of heat under formulated laws, so that the phenomena are all theoretically explained as soon as the experimental data from which the laws were deduced have been accounted for by reasoning founded upon the *à priori* principles of the Newtonian Philosophy.

The theory proposed by Ampère to account for magnetic attractions and repulsions by an agency analogous to that of galvanic currents in coils, is so far properly theoretical that, if experimentally verified, it would contribute something towards *understanding* the intrinsic nature of magnetism. But, as I have intimated in page 98, there is reason to question whether that theory be not contradicted by experiment; and it is, moreover, to

be said that all the magnetic phenomena which are considered to be accounted for by referring magnetic force to galvanic agency, can be explained by the hydrodynamical theory of magnetism, and that according to the principles of this theory magnetic currents cannot be composed of galvanic currents.

A method of philosophy wholly different from, if not inconsistent with, that I have been advocating, is adopted by some physicists of the present day. It consists in deducing explanations of phenomena from *general laws* (improperly called *principles*), such as the law of Vis viva, and that which is called "the conservation of energy." With respect to the law of Vis viva, we know that it is capable of being expressed by a formula arrived at by mathematical reasoning, and that the reasoning consists in forming according to dynamical principles, and integrating, general differential equations comprehensive of all particular cases. By being thus *formulated* the law of Vis viva has become matter of science, and it does not appear that it could have been made such by any other process. Being demonstrated by this inductive reasoning, it may be applied deductively in the solutions of particular problems.

Any general law which is similarly applied in accounting for *physical phenomena*, requires to be analogously arrived at by inductive reasoning, before it can be legitimately and definitely so applied. I fail to see that this has been done with respect to the above-mentioned law of the conservation of energy. I even venture to say that no such law admits of being demonstratively established excepting by reasoning on the basis of the Newtonian principles of philosophy. The results I have arrived at,

by the adoption of those principles, respecting the qualities and agency of a universal æther, are clearly adapted to giving reasons for the "correlation" and "transformation" of forces, considered to be indicated by experiment, and for the conservation of uniformity in the total energy of the universe.

I take occasion to state here that I have adhered to the term "Hydrodynamics" in preference to "Hydrokinetics," not merely because it is established by long usage, but chiefly because it may be taken to imply, although not expressly significant of motion, that the department of science it designates treats of motions as necessarily having relation to *force*. The word "motion" has been used by some eminent physicists with so little reference to the essential distinction between its signification and that of "force," that, as I am able to testify, others have thence been led to infer that motion is capable *per se* of producing motion; whereas it is a fundamental axiom of natural philosophy that motion cannot be generated by motion itself any more than by the negation of motion. The following is an instance of disregard of the distinction between force and motion. The boring of a cannon, which may well be called a *tour de force*, is known to produce heat; and hence, taking into account the law of the *mechanical equivalence of heat* established by experiment, it might with reason be affirmed that heat is a *mode of force*. Instead of which, from this and like experiments the inference is drawn that heat is "a mode of motion," an expression implying that motion is *per se* operative. In order not to give occasion to such misconception of the quality of motion, I have avoided substituting for "Hydrodynamics" a

term not well adapted by its etymology to make a distinction between force and motion.

The foregoing particulars relative to the philosophical views and productions of recent date will serve to indicate on what grounds I said (p. xvi.) that I could refer to no one who had treated of the physical forces *theoretically*, that is, in accordance with the principles of Newton's "foundation of philosophy." Modern physical science is characterized by great and successful efforts to extract laws of greater or less generality from the results of observation and experiment, and it is worthy of remark that the experimental demonstration of a law is generally accompanied by some speculation on the part of the experimenter as to its *cause*. Such speculations, although they do not supersede the necessity for a mathematical theory, are of assistance in carrying on experimental research, and may be regarded as testifying that experimentalists themselves look upon physical science as something more than the mere determination of laws. But the demonstration of physical laws by mathematical reasoning founded on necessary and intelligible hypotheses, in other words, the prosecution of the Newtonian Philosophy of Causation, has scarcely been attempted since Newton's time either in England or on the Continent. In this respect the work I published in 1869 on the *Principles of Mathematics and Physics* stands alone, and has placed me in a kind of isolation relatively to my scientific contemporaries. Still I maintain that its contents, so far as they are true, give a legitimate extension to the course of theoretical philosophy that Newton began; and more than this, it may be asserted that works of this kind, devoted

exclusively to doing what is to be done by reasoning, are absolutely necessary for advancing and completing physical science, inasmuch as for this end that which can be effected by reasoning alone is just as necessary as that which can be effected by experiment alone.

I am well aware that in so large an undertaking errors of detail, if not of principle, may have been admitted, and that there is also much which has been very imperfectly accomplished. In fact, various errors have been corrected, and improvements indicated, in the present work, which was written chiefly for the purpose of forming a kind of appendix to the larger one, and thereby making it more trustworthy and complete. With time and strength at disposal I should be able to make many more improvements.

Notwithstanding errors and imperfections, the contents of the two works will, I think, suffice for establishing eventually a claim to having initiated an advance in the application of mathematical reasoning to physics, having the same relation to the existing state of physical science as Newton's new application of mathematics had to the science of his day, and adapted in like manner to inaugurate a new scientific epoch.

In concluding this Introduction I propose to say a few words on the bearing which the philosophical views I have advocated may possibly have on the mathematical studies of the University of Cambridge. I have argued that the superstructure of physical science is raised by two essentially different means, by *experiment* and by *mathematical reasoning*, and that for making it complete, it is absolutely necessary to employ them in combination. The latter of these means has long been amply promoted at

Cambridge by the facilities there afforded for acquiring an accurate and extensive knowledge of the different departments of pure and applied mathematics, while for an equally long period there was great deficiency as to the means available for becoming acquainted by experiment with physical facts and laws. Happily this want is now being supplied by the erection of new buildings, and the provision of appropriate apparatus, for oral and practical instruction in experimental physics. It may be that as far as regards instruction merely by instruments and experiments students at Cambridge will not have greater advantages than those at other educational institutions; but because there is no place in the world at which mathematics are more efficiently and extensively studied, probably at no other place could the study of *theoretical* physics, as requiring the union of mathematical reasoning with experimental research, be more effectively prosecuted.

Under these circumstances it is to be desired that the studies of the University should be directed more than formerly towards physical subjects, and that questions on such subjects should be introduced in greater degree into the examinations for mathematical honours. It may also be hoped that a proportion of Cambridge mathematicians who have had this direction given to their studies will not be content with merely acquiring a knowledge of physics, but will be induced to employ the mathematical and experimental means at their command in making efforts to extend the boundaries of physical science.

But as respects any endeavours to make advances in physical knowledge by mathematical theory, Newton's

"foundation of all philosophy," the claims of which to recognition I have so long urged, and now urge again (perhaps for the last time), is indispensable, and cannot without detriment to scientific truth be superseded by any other.

CAMBRIDGE,
June 6, 1873.

THE

MATHEMATICAL PRINCIPLES OF PHYSICS.

IN order to convey a precise idea of the purpose of this publication, I think I cannot do better than begin with stating what the title signifies, and for what reasons it has been chosen.

The full title of Newton's *Principia* is, "The Mathematical Principles of Natural Philosophy," and, accordingly, not only are the principles of Physical Astronomy mathematically demonstrated in that work, but the principles of other departments of Natural Philosophy are to a considerable extent adverted to, with reference, apparently, to the possibility of their being eventually verified, like those of Physical Astronomy, by mathematical reasoning. Newton's Treatise on *Optics* consists essentially of the determination of physical laws by *experiment*, and on that account does not properly come under the head of mathematical principles of Philosophy. It is true that in an Appendix to the *Optics* Newton has expressed à priori views and conjectures relative to various physical subjects; but he evidently felt that the physical science of his time was inadequate to furnishing the means of testing his theories by calculation and experiment, and he has consequently put them in the form of "Queries." In the *Principia*, however, especially at the beginning of the Third Book, entitled "De Mundi Systemate," certain views are introduced which have immediate relation to the fundamental principles on which alone mathematical reasoning can be applied to Physics in general. These views I shall have occasion to take account of in the course of this Essay. At present it will suffice to state

that I speak of the Mathematical Principles of Physics with the same comprehension as to subjects, and the same limitation to those that are susceptible of à priori mathematical treatment, as is implied by the full title of the *Principia*. The subject of the Essay might also be named "Theoretical Physics," inasmuch as it relates exclusively to the establishment of physical theories by mathematical reasoning.

Physical Science, or, as for brevity it may be called, "Physics," embraces a wide range of subjects, as a bare enumeration of those which are proposed to candidates for Honours in the Examination in this University for the Natural Sciences Tripos will sufficiently shew. These are, Botany, Comparative Anatomy, Physiology, Zoology, Geology and Palæontology, Mineralogy, Chemistry; and, in a different category, Sound, Light, Heat, Electricity, Galvanism, and Magnetism. Of these subjects, those of the first kind, whatever form they may eventually receive, have no pretension in the actual state of physical science to be considered as admitting of à priori mathematical treatment; those of the other kind have already been brought, in different degrees, within the domain of mathematics; and mathematical theories, not only of Sound, but also of Light, Heat, Electricity, Galvanism and Magnetism, have been proposed. A mathematical theory of Chemistry is not at present possible because it requires an antecedent determination of the laws and conditions of action of atomic and molecular forces,—a problem not to be regarded as insolvable, but as not yet solved. For this reason also a mathematical theory of Mineralogy is not attainable, so far as relates to the causes determining the interior arrangement of atoms, and the dependence thereon of the forms of crystals, although Crystallography, considered as the science of the laws and classification of the external forms of crystals, consists for the most part of geometrical relations susceptible of exact mathematical treatment. Not until the theories of the physical *forces*, Light, Heat, Electricity, &c., have been brought much nearer to perfection than they are at present, will it be possible to enter upon mathematical theories of Chemistry and Mineralogy.

From the above statement respecting the characteristics of the different physical subjects, it will be seen that the study of Physics is two-fold; it is, first, the study of facts, laws, and external relations, as ascertained by experiment and observation; and then the study by the aid of mathematics of *reasons* for the known facts, laws, and relations. All the subjects of each of the two classes above mentioned admit of the first kind of study; but those of the second class admit of being also studied mathematically. The latter study is clearly of a higher grade than the other. At the same time to acquire a knowledge, especially if it be by practical means, of the phenomena of Botany, Zoology, Geology, and Chemistry, together with those of Heat, Electricity, and Magnetism, and to deduce therefrom laws, relations, and classes, is doubtless an efficient means of mental discipline, and consequently, apart from the economic uses to which such knowledge may be applied, those subjects have with good reason been selected for the reading of candidates for the *ordinary* B.A. degree. But the study of *mathematical physics*, although necessarily preceded by, and dependent upon, a knowledge of phenomena and their laws, aims at acquiring in addition a knowledge of the *causes* of phenomenal laws, and, being such, can only be conducted by means of mathematics. Moreover, the mathematical reasoning required for this purpose is of an advanced kind, more comprehensive and more complex than that employed in Physical Astronomy. For these reasons the discussions of physical principles and problems contained in this Essay, so far as they bear on the Cambridge mathematical studies, will have reference almost exclusively to the studies of Candidates for high mathematical Honours.

1. Before entering upon particular considerations it will be proper to introduce a general enquiry respecting the course to be pursued in the application of mathematics to physics. What, in short, is the true method of the theoretical department of natural philosophy? On this point physicists in the present day maintain different views.

2. For answer in part to this enquiry I would say that every

application of mathematics in physics necessarily rests on *hypotheses*, that is, as the word hypothesis implies, on *foundations* of reasoning. In order to illustrate this position with respect to physics, it will first be shewn that every application whatever of mathematics to questions in which force is concerned rests on hypotheses.

3. In Statics the hypotheses are, (1) that forces acting on a rigid body at rest are equilibrated by other forces; (2) that the effect of a force on a rigid body is the same at whatever point in the line of its direction it be applied. The Proposition called "the parallelogram of forces" is established on these two hypotheses, whether it be proved by means of properties of the Lever, or in Duchayla's method, or be inferred from the general equation of Virtual Velocities.

4. The hypotheses of Rigid Dynamics and the reasoning founded upon them become, by the intervention of D'Alembert's Principle, the same as those of Statics.

5. D'Alembert's Principle is not usually enunciated in its simplest and most comprehensive form, as will, perhaps, be seen from the following argument. It is conceivable that the whole of any system acted upon by forces, whether it be rigid or not, may have *in space* at any instant a motion exactly equal and opposite to the motion of a given element of the system at that instant. On this supposition the element will at that moment be absolutely stationary. If subsequently the effective accelerations of the element be impressed on *all* the parts of the system in the directions opposite to those in which the accelerations are actually produced, the element will remain at rest, and no change will be made in the relations of the parts, because all will have been submitted to the same action in the same direction. But evidently under these circumstances the equilibrium of the element results from the counteraction of externally impressed forces, together with tensions and reactions of fixed obstacles, by the reversed effective forces. The same argument may be applied to every other element. Hence D'Alembert's Principle is true with respect to the motions of the whole system. In this way the

application of the principle is proved to be admissible whether the parts of the system be perfectly rigid, or elastic, or imperfectly fluid, or perfectly fluid.

6. The hypotheses of Hydrostatics are, (1) that the parts of fluids *press* against each other and against any rigid bodies with which they are in contact; (2) contiguous parts of perfect fluids are separable one from another by an indefinitely thin solid partition without employing any assignable force. It is an immediate inference from these properties, that the internal pressure of a perfect fluid is in the direction *normal* to *any* plane conceived to be drawn in it. For a thin solid partition, put in the place of the plane, would necessarily be pressed normally, and as the partition might be withdrawn without employing assignable force, the pressure would be the same after the withdrawal as before. Another inference from the same properties is, that any element is *moveable* by the application of the least assignable force; because, as there is no obstacle to partition, there is no obstacle to motion.

7. The appellation "Hydrodynamics" being employed with respect to fluids in the sense in which, by established usage, "Rigid Dynamics" is applied to solids, it may be asserted that the hypotheses of Hydrodynamics become the same as those of Hydrostatics by the intervention of D'Alembert's Principle taken in the general acceptation above indicated. By reason of the property of perfect mobility of parts, the motion of a fluid depends only on the mutual pressure of the parts, in addition to the action of extraneous forces.

8. The *law* of the equality of the pressure of a perfect fluid in all directions from a given position is deducible by a known process from the hypotheses above stated, and, by the application of D'Alembert's Principle in the extended sense already explained, the process of deduction is the same in Hydrodynamics as in Hydrostatics, and the evidence for the law is as valid in the one case as in the other. Some mathematicians have doubted, it seems to me without reason, whether the law of equality of pressure is capable of deductive proof for fluid in motion. This

law holds, in fact, with respect to Hydrostatics and Hydrodynamics, the same place as the parallelogram of forces, with respect to the Statics and Dynamics of rigid bodies, and both the one and the other are deducible by *reasoning* from the respective hypotheses apart from experiment, while at the same time experiment confirms the truth of the deductions.

9. The hypotheses on which reasoning relating to elastic, or partially fluid, bodies is based are *immediately* given by experiment. For instance, experiment furnishes the data which connect extension with stress or tension. The vibrations of an elastic cord present a familiar example of problems of this class. These experimental hypotheses may be such as to admit eventually of à priori deduction, but the reasoning required for this purpose belongs to questions of a different order from those now under consideration.

10. It remains to state what are the hypotheses of Physical Astronomy. This is to be regarded as a department of Dynamics not included in Rigid Dynamics, because, if the problems of Precession and Nutation and of Tides be excepted, all the rest relate virtually to the motions of single particles acted upon by the force of gravity. The hypotheses of Physical Astronomy are (1) that every portion of matter attracts towards itself every other portion; (2) that the amount of attraction varies inversely as the square of the distance between the attracting and attracted particles. It might seem that the law of the inverse square admits of being deduced from hypotheses, and on that account cannot itself be regarded as an hypothesis. This, however, is not the case. That law is not deducible by reasoning alone, nor by observation alone, but is proved to be true by a combination of both. Before Newton's time no one was able to demonstrate the law of decrement of the Earth's gravitation with increase of distance from the centre, because no one knew how to make the calculation appropriate to the action of a variable force. Newton proved by his method of prime and ultimate ratios that such action is measured by the second differential coefficient of linear space with respect to time. If the Earth had not been attended

by a satellite, the law of the inverse square might have been demonstrated by means of Kepler's observational Laws, but not until Newton had effected what is in fact the solution of a differential equation of the second order between two variables. The *tout ensemble* of Physical Astronomy furnishes, by being combined with results of observation, the proper proof of the truth and generality of the two above-mentioned hypotheses.

11. It may here be remarked that this demonstration of the truth of the hypotheses of Physical Astronomy establishes nothing respecting the essential quality of gravitating force. It would be quite gratuitous to infer from it that the elementary portions of visible and tangible substances are indued with any attractive virtue. Newton expressly deprecated drawing any such inference. His theory of gravitation gave rise to much discussion about "occult qualities," and eventually, notwithstanding his disclaimer, there prevailed a belief in the occult quality of gravitation; neither has it ceased to prevail at the present time. Yet the proof of the hypotheses of Physical Astronomy warrants no other conclusions than that by some means or other the elementary parts of bodies are mutually attracted, and that, whatever the nature of the means may be, the descent of a body towards the earth's surface is produced by the very same. The fact alone that gravity decreases with distance from the attracting body is, according to views expounded by Newton in his Regula Tertia Philosophandi, evidence sufficient that it is not an essential quality of bodies. The fact also that Physical Astronomy, by demonstrating the law of the inverse square, has determined that the law of decrement admits of precise quantitative expression, is presumptive evidence that this, like every other quantitative law, may eventually be deduced by mathematical reasoning from ulterior principles.

12. The foregoing statements may serve to justify the assertion that the mathematical treatment of all questions of the *ordinary* kind relating to the effects of forces rests on hypotheses. It may next be remarked that hypotheses that are made the bases of mathematical reasoning can only be such as are *intelligible* from

observation and experience. This quality belongs to the hypotheses of Statics, Rigid Dynamics, Hydrostatics, and Hydrodynamics, simply for the reason that they are either immediately given, or are suggested, by observation and experiment. The hypotheses on which the treatment of problems relating to elastic solids and partially fluid substances is based are known only by experimental determinations, and on that account are necessarily intelligible. Those of Physical Astronomy might seem to be exceptional in this respect. But when it is considered that we have only to conceive of the force of gravity as varying in amount in a definite manner with the distance from the attracting body, whilst in quality it is always the same as that force we are familiar with by witnessing the acceleration of bodies falling towards the earth's surface, the hypotheses of Physical Astronomy may be pronounced to be intelligible from experience and observation.

13. Besides the hypotheses of a general character already mentioned there are others of a more specific kind, by which one substance is distinguished from another of the same class. Thus solids, whilst they differ essentially from fluids in not allowing of the motions of parts *inter se*, may be divided into those that are perfectly rigid, and those which by the application of force admit of some degree of relative displacement of parts, and are consequently more or less *elastic*. There is reason to say that all solid substances are in some degree elastic, and that perfect rigidity is a hypothetical abstraction. Similarly, besides the hypotheses which define all fluids of perfect fluidity, there are others which distinguish one perfect fluid from another. Thus water is assumed to be incompressible; the pressure of air of given temperature is supposed to vary proportionally to its density. Neither of these qualities is determined experimentally to be exact, but both are shewn by experiment to be very approximately true, and for that reason the hypotheses of their being exactly true are intelligible foundations of mathematical reasoning. In short, a fluid possessing the properties of perfect divisibility and mobility is an abstraction, all the fluids in nature being such that parts that are

in motion are subject to some degree of frictional action or viscosity.

14. As to the kind of *mathematical treatment* which the subjects above mentioned severally require, it may be said generally that it depends on properties of the substance acted upon by the given forces, and on the quality of the hypotheses by which the properties are defined. In Statics the forces are measured by weight, and the element of time is absent; in consequence of which the calculations are conducted algebraically, and do not require the solutions of differential equations. In Rigid Dynamics, on the contrary, time is an essential element, the effective forces being measured by differential co-efficients of space with respect to time, and consequently the answers to questions of this class involve the solutions of differential equations. The differential equation is always in the final analysis one of the second order between *two* variables. As the bodies constituting the system are all rigid, each elementary portion of any one body retains throughout the motion the same position relative to all the other elements of that body; on which account, when the mutual relations of the different bodies are known from the given conditions of the problem, the differential equations applicable to individual elements are always reducible to a single differential equation between a space-variable and a time-variable.

15. In Physical Astronomy, if we except the problem of the Tides, the differential equations are all of the same class as those in Rigid Dynamics, because either the bodies are capable of being treated as single particles, or if treated as extended masses, as in the problem of Precession and Nutation, they are assumed to be rigid. So far, however, as the assumption of rigidity is not exact, the solution of that problem has to be effected by means of differential equations involving more than two variables.

16. Although when fluids are in equilibrium each element of a mass retains its position relative to the other elements just as if the mass were solid, the mathematical reasoning in Hydrostatics involves *partial* differential equations, because, as the elements

have no rigid connection, they are *susceptible* of relative displacement by the least possible disturbance.

17. In Hydrodynamics the pressure and velocity are functions of co-ordinates and the time, and the differential equations proper for determining their values may contain five variables, and cannot contain less than three. The circumstance that the number of variables is not in any case reducible to two makes the process of applying the integrals of differential equations in Hydrodynamics distinctly different from that which is required in Rigid Dynamics and Physical Astronomy, inasmuch as the integrals in the former application contain arbitrary *functions of variables*, and those in the latter arbitrary *constants*.

18. In another respect also the mathematical treatment of the motion of a fluid mass differs from that employed in Rigid Dynamics. The elementary portions of a fluid are capable of motion *inter se*, and, in consequence, each element is in general changing its form. It is clearly necessary that this peculiarity in the condition of fluids in motion, to which there is nothing corresponding in the condition of moving solids, should receive an analytical expression. For this purpose we must conceive to be drawn in the fluid a surface (which may be called "a surface of displacement"), cutting at right angles the directions of the motions of the elements through which it passes, and thence infer the general differential equation of such surfaces. That equation, as may be readily seen, is obtained by equating to zero the differential function $udx+vdy+wdz$, the factors u, v, w being the resolved parts, in the directions of the rectangular axes, of the velocity at the point xyz at any time. In order to express the condition that the elements are changing form, *it suffices to assume that function to be an exact differential;* for in that case the lines of motion are normal to surfaces of displacement which are continuous through either a finite, or indefinitely small, extent, and the change of form of each element results from the convergency or divergency of these lines. There are, however, cases of the motion of a fluid mass in which the elements do not change form; a rectangular element, for instance, remaining rectangular

during the whole of the motion, either because the mass moves as a whole as if it were solid, or consists of ultimate parts which individually so move. In such cases the above differential function is integrable by a *factor*.

19. Besides obtaining the general differential equation of surfaces of displacement, it is necessary to express analytically the condition that such surfaces may be drawn through all the elements at all times. This is done by simply equating to zero the variation with respect to space and time of the above-mentioned differential equation; by which process a fundamental hydrodynamical equation is obtained which expresses that the motion of the fluid, whether the elements change form or not, is such as to satisfy necessary relations of *time and space* independently of the action of forces.

20. Again, fluids in motion, in common with solids, are subject to the condition that the quantity of matter is *invariable*. In rigid bodies this condition, which is insured by their very constitution, requires no analytical expression. But with respect to fluids, inasmuch as the condition of constancy of mass partly determines the relative motions of the parts, it is necessary to express analytically that that condition is always and everywhere maintained, notwithstanding changes of form of the elements, and changes of their relative positions, by the motion. On this principle a second fundamental equation of Hydrodynamics is formed. This equation, combined with the first, insures that the motion is consistent with necessary relations of *space, time, and matter*, independently of the action of forces.

21. For calculating the effects of impressed forces and of the pressure of the fluid, it is required to combine a third general equation with the two preceding. This third is essentially a *dynamical* equation, and is immediately formed by employing D'Alembert's Principle in the manner already explained. The solution of every hydrodynamical problem either expressly or virtually takes into account all three equations.

22. It is not necessary for my purpose to remark further at present on the principles and processes of the mathematical

reasoning in Hydrodynamics. Important applications of this department of applied mathematics will come under consideration at a subsequent part of this Essay. I shall now only add that the deduction of inferences from the mathematical reasoning in Hydrodynamics presents difficulties, both as to principle and detail, which have not yet been overcome, and that two problems of special interest, one relating to Tides and Waves, and the other to vibrations of the Air, cannot be said to have been solved with complete success.

23. It remains to mention the class of questions relating to elastic solids and imperfect fluids. As the parts of these bodies undergo in greater or less degree relative displacements under the action of forces, their movements can be mathematically determined only by means of partial differential equations containing not fewer than three variables. I forbear saying more on problems of this class, as I shall not have occasion to refer to them in the course of the subsequent discussions.

24. After the foregoing consideration of the principles of the ordinary classes of dynamical problems, which, as their mathematical treatment rests on experimental data, might be said to be the problems of *experimental mechanics*, we may proceed to an analogous consideration of questions of a higher order, namely, the dynamical theories of Light, Heat, Gravity, Electricity, Galvanism, and Magnetism. These theories require for their establishment the solutions of dynamical problems differing essentially from those of the ordinary kind, inasmuch as their mathematical treatment rests exclusively on *theoretical* hypotheses, and the problems might on that account be said for distinction to be those of *theoretical physics*. The distinction will, perhaps, be made more evident by the remark that the object of this higher class of dynamical questions is to explain, among other experimental facts, those especially which constitute the fundamental hypotheses of experimental mechanics. If these facts be accounted for, all those which are proved to be consequences of them by the solutions of the problems of experimental mechanics would be inclusively explained by the science of Theoretical Physics.

25. It will be necessary to begin with pointing out certain general principles equally applicable to all the above-mentioned departments of physics, and then to discuss separately and in order their mathematical theories. Two rules, suggested analogically by the tenor of the antecedent remarks, may be laid down as applying of necessity to theoretical physics.

First, the investigations must commence with *hypotheses.*

Secondly, the hypotheses must be such as are *perfectly intelligible* from sensation and experience, and proper for being made foundations of mathematical reasoning.

26. It is requisite, therefore, before entering on any other considerations, to answer the question, What are the necessary hypotheses of Theoretical Physics? The answer I propose to give to this question will be in strict accordance with Newton's principles of Natural Philosophy, and especially with those enunciated in the Regula Tertia at the beginning of Book III. of the *Principia.* (These are the "views" referred to at the commencement of this Essay.) Newton there maintains that the qualities of bodies which are universal, inseparable from them, and admit neither of diminution nor increase, can only be known by experience, and that consequently no other qualities can be attributed to the least parts of bodies than *sensible* qualities, those, namely, of which we have become cognisant by sensation and experience relative to *masses.* On this principle he asserts that "all the least parts of all bodies are extended, hard, impenetrable, moveable, and indued with vis inertiæ." "And this," he adds, "is the foundation of all philosophy[1]."

27. In all the researches I have published on Theoretical Physics I have adopted the Newtonian hypotheses, with only the following modifications. I assume that the least parts of bodies are not only "undivided" but absolutely indivisible; in short, that they are *atoms:* and in place of saying that they are "hard and impenetrable" I attribute to them the equivalent quality of constancy of form and magnitude, admitting at the same time

[1] "Hoc est fundamentum totius philosophiæ." The word "fundamentum" is the Latin equivalent of *hypothesis.*

that different atoms may be of different magnitudes. Moreover I assume that all atoms have the spherical form. These modifications and additions have been introduced with the view of making the Newtonian hypotheses appropriate bases of modern mathematical reasoning. They conform to the condition of being intelligible from experience, and, besides, are not unsupported by experimental evidence. For the phenomena of Chemistry and Mineralogy clearly point to indivisibility and constancy of magnitude as properties of the ultimate constituents of bodies, and the motion *inter se* of the elementary parts of incompressible fluids seems to indicate that the atomic components, from being spherical, are indifferent as to the directions in which they are turned towards surrounding space.

28. But Theoretical Physics requires hypotheses to be made not only respecting the qualities of the ultimate constituents of bodies, but also respecting the nature of the forces and agencies by which the supposed spherical atoms are maintained in equilibrium, or are set in motion, or, in short, mutually attract and repel. Considering the state of mathematics and physics in Newton's time, it is not to be expected that much concerning these questions can be gathered from his writings. However, in the very last paragraph of the *Principia* views are expressed which, as being remarkably in accordance with those I shall presently advocate, it will not be inappropriate to quote here:

"It would now be proper to add something concerning a certain very subtle spirit pervading gross bodies, and latent in them; by the force and actions of which the particles of bodies mutually attract at very small distances, and coming into contact cohere; and electric bodies act at greater distances, both by repelling and attracting neighbouring small bodies; and light is emitted, reflected, refracted, inflected; and bodies are warmed; and all sensation is excited; and the limbs of animals are moved at will, by vibrations, namely, of this spirit propagated through the solid fibres of the nerves from the external organs of the senses to the brain, and from the brain to the muscles. But these things cannot be briefly explained, neither is there a sufficient amount

of the experiments required for accurately determining and demonstrating the laws of the actions of this spirit[1]."

29. Not only was there a deficiency of experiments, but the mathematics of the time was inadequate to the performance of the calculations necessary for bringing such views concerning the physical forces into comparison with experimental facts. It is clear, however, from the above passage that Newton recognised the existence of a rare elastic medium, capable of pervading grosser bodies, and that he regarded light, heat, and electricity as modes of its action. Modern advances in experimental physics have pointed to the conclusion that these forces, together with galvanism and magnetism, are all mutually related by reason of some common circumstance, or condition, and accordingly it would not be without the support of antecedent experience to assume that they are all modes of action of the *same* medium. But in order to test the truth of this conception it is necessary to make respecting the medium certain hypotheses, from which its modes of action may be deduced by analytical calculation. The actuality of the hypothetical medium and the truth of the general physical theory might then admit of being satisfactorily established by comparisons of a large number of facts in each of these departments of physics with the results of calculation. The universal agent I call *the æther;* this appellation, which, as well as *atom,* has descended to us from very remote antiquity, being now very generally adopted.

30. These hypotheses respecting the quality of the medium must, like all other physical hypotheses, conform to the rule of being perfectly intelligible from antecedent experience, and at the

[1] The idea that some intervening medium transmits light from one point of space to another, and that this medium is a "subtle fluid" filling "the pores" of bodies, and "extending from the stars to us," was decidedly entertained by Descartes. (See Sects. II., III., and VII. of Chap. I. of his *Dioptrics*). This view was accepted by Newton and Huyghens, and seems to have become in their time a settled philosophic dogma. In his letter to Boyle, Newton says, "I suppose that there is diffused through all places an ætherial substance, capable of contraction and dilatation, strongly elastic; in a word, much like air in all respects, but far more subtile." (Horsley's *Newton*, Vol. IV. p. 385.)

same time admit of mathematical reasoning being founded upon them. Accordingly in my published researches respecting the modes of action of the æther, I have assumed that, as far as regards the theories we are about to consider, it is in effect *continuous*, and capable of being treated mathematically like air of given temperature, whatever may be its actual intimate constitution. I have, besides, assumed that it is an extremely rare and elastic fluid, that it fills all space not occupied by the atoms of visible and tangible substances, and that its pressure is always and everywhere in a constant proportion to its density. Being, by hypothesis, the source of all physical action, it must also be supposed to be throughout of the same density, so long as it is not disturbed. According to the above statement, the mathematics required for carrying on these researches are the same as those of Hydrodynamics, the principles of which have been already considered in the previous part of this Essay[1]. (See Arts. 17—21.)

31. It will be seen that a general physical theory established on these foundations would demonstrate that all the forces of nature are *pressures*. Here an important remark is to be made. If we had only the sense of *sight* to guide us, we might conclude that bodies have the faculty of acting dynamically on other bodies at a distance without any intervening means or agent. But by the sense of *touch* we have a precise idea of contact as distinct from non-contact, of pressure by contact, and of pressing as a *personal* act. The indications thus given us by the sense of touch supply what is defective in the indications of the sense of sight, and must of necessity be taken into account in a philosophical enquiry respecting the quality of physical agency. We have, in fact, no other understanding of the action of one body on another

[1] The science of Analytical Hydrodynamics, unknown in Newton's time, is due to the labors of Euler, Lagrange, Poisson, and other continental analysts, English mathematicians having had scarcely anything to do with it. When I commenced researches in theoretical physics, this instrument for conducting them was ready at hand, and there was not the same necessity to find out the proper modes of applying partial differential equations in Hydrodynamics, as that Newton was under to discover the calculation appropriate to Physical Astronomy. Some modifications, however, of what had previously been done, and additions thereto, I found to be indispensable.

than that we acquire by the feeling experienced when the hand is voluntarily pressed against any substance. We may at the same time be ignorant of the exact physical conditions under which the sensations of contact and pressure are experienced. The sensations themselves are universally perceived and understood, and for that reason the hypothesis of pressure by contact may be legitimately admitted into a system of theoretical physics, the hypotheses of which, according to the second rule enunciated in Art. 25, must be intelligible from sensation and experience. The unknown physical conditions of the sensations may eventually be discoverable by means of this very course of philosophy.

32. Again, it may be asserted that if all the physical forces be resolvable into pressures of a perfect fluid, it may be possible to obtain positive knowledge of the modus operandi of each, because the motions and pressures of such a fluid can be calculated on the principles of Hydrodynamics. Science so attained is *Theory* in the strict sense of the word, inasmuch as it consists of an *intelligent comprehension of causes.* Prior to any considerations respecting the essential quality of physical force, knowledge to a great extent of the *laws* of physical operations may, as the history of science has shewn, be acquired by experiment and calculation; and this, in the opinion of some physicists of the present day, is the whole of philosophy. On the contrary, this is only the experimental part of philosophy, as distinct from the theoretical, and the totality of philosophy consists of the two parts combined in certain relations. Theory necessarily rests on data from observation, and at the same time has to account for laws which have become known by observation.

33. The foregoing general considerations may serve to shew why on the hypothesis that the æther is the source of all physical force, it must, in order that its action may be intelligently investigated, be supposed to act by pressure. But here another consideration comes in. Just as in the case of any other fluid, the parts of the æther mutually press, and also press against the solid atoms with which they are in contact. But, by hypothesis, the atoms are of invariable form and magnitude. Hence there will

be called into action by the pressure of the æther a definite resistance at the surfaces of the atoms, which is itself a physical force distinct from such pressure. Accordingly in nature there are two kinds of force, the *active* pressure of the æther and the *passive* resistance of the atoms. Both these are taken into account, as well as the inertia of the atoms, in the problems relating to the physical forces to which attention will presently be directed.

34. It may be urged that although this system of physics may account for other laws, it does not account for the law that the variations of the pressure of the æther are in exact proportion to the variations of its density, nor, in fact, for the density being susceptible of variation. To this objection I reply that these qualities of the æther are to be reckoned among the *hypotheses* of the system, having been already enunciated as such (Art. 30), and that all we are concerned with at present is, that the assumed qualities should be intelligible from common experience. The suppositions of variability of the pressure and of its varying proportionally to the variations of the density satisfy this condition. I shall have occasion in the sequel to revert to this point.

35. According to the principles advocated in the immediately preceding paragraphs, it is evident that *Gravity* cannot be regarded as an essential quality of bodies. Newton in his Regula Tertia expressly states that "he by no means affirms it to be such"; and elsewhere in his writings he asserts that no one competent to the understanding of philosophy could hold such an opinion. The general physical theory I have proposed would be altogether at fault if it failed to indicate a mode of pressure of the æther which might account for the force of gravity, and the law of its variation with the distance from the point of emanation; and accordingly this problem will receive, in its proper place, particular consideration.

36. I now go on to the application of the foregoing general principles to the theories of the different physical forces, taking them in the order, Light, Heat (inclusive of atomic and molecular forces), Gravity, Electricity, Magnetism, and Galvanism. The reasons for adopting this order will appear in the course

of the general argument. In a publication like the present it would not be possible to enter upon the details of the mathematical reasoning applicable to each theory; for these I beg to refer to the work I have published on "The Principles of Mathematics and Physics." All that I purpose to do is, to discuss the methods of solving certain selected problems, with reference specially to the essential principles of the respective theories, in order to point out the characteristics and limitations of the theories, and to what extent they may be considered to have been successfully handled.

LIGHT. 37. It will appear from what will presently be said, that the theory of light to which the following remarks apply may properly be called an *Undulatory Theory.* The phenomena proposed to be explained by it may be divided into two classes; those resulting immediately from qualities of the medium in which the light is generated and transmitted, and those depending on the relations of light to the ultimate constituents of visible and tangible substances.

The phenomena, or facts, of the first class are principally these:—

The rectilinear transmission of light through space; its taking the form of rays; the uniformity of its rate of propagation in space; difference of intensities of different rays, and non-dependence of rate of propagation on intensity; variation of intensity according to the law of the inverse square by divergence from a centre; composite character of common light, and resolvability into parcels of different intensities; distinction by phase; distinction by colour; resolvability into parcels of different colours by spectrum analysis; transmutability of rays; colours resulting from compounding lights of different colours; non-interference, in general, of parcels of light from different sources; interferences under particular circumstances; distinction between common light and polarized light; resolution of common light into equal portions polarized in rectangular planes; non-interference of rays oppositely polarized; non-dependence of the combined intensities of two oppositely polarized rays having a common path on the difference of their phases;

differences, in certain proportions, of the intensities of the parts of a polarized ray resolved by a new polarization; distinctive characters of plane-polarized, elliptically-polarized, and circularly-polarized light; limited lateral divergence of rays, and phenomena of Diffraction.

38. It is to be observed that although the *phenomenal exhibition* of some of these properties, as especially colour, polarization, and diffraction, is dependent upon certain relations of the æther to the constituents of the grosser substances, the properties themselves exist independently of such relations, being referable exclusively to qualities of the medium in which light is generated and transmitted. Now respecting this medium no other hypotheses have been made than that it is a very rare and elastic continuous fluid, of uniform and constant density and pressure when at rest, and pressing always proportionally to its density when in motion. Consequently the theoretical explanations of all the above-named properties are to be deduced solely from these hypotheses, by reasoning conducted strictly according to the rules of abstract mathematics; neither can they be obtained in any other way, if the proposed method of philosophy be true. Here then is an experimentum crucis of the severest kind for testing the truth of this method. I proceed therefore, in the next place, to indicate in what way, and with what limitations, theoretical explanations of the essential properties of light are obtainable by that process.

39. In many respects the properties of light have their exact analogues in properties of *sound*, and this circumstance is clearly in accordance with the hypothesis that æther is, in effect, constituted like air of given temperature, having the same relation between density and pressure. *Loudness* of sound corresponds to *intensity* of light, *pitch* of musical sounds to *colour*, co-existence of musical sounds to co-existence of parcels of light from different sources; and so on. In short, from the supposition that the fundamental qualities of the two media are exactly alike, it necessarily follows that the consequences which flow from the qualities are analogous in *all* respects. The fact that apparently this is not altogether the case is referable to the great disparity between the

æther and the air as to tenuity and elasticity, the æther being capable of pervading the interstices between the atoms of air and all other bodies, and of propagating disturbances at a rate about 870,000 times greater than that in air. So far as the properties of light and sound are strictly analogous, they are accounted for by means of the same course of mathematical reasoning; which, as being given in the Elementary Treatises on Hydrodynamics, I need not advert to here.

40. The properties of light the analogues to which in sound are wholly or very nearly imperceptible are, chiefly, these: its being composed of *rays;* dynamical action in directions transverse to the axes of the rays; susceptibility to polarization; and the limited lateral divergence on which the phenomena of diffraction depend. Now the argument carried on thus far demands that these properties, as well as those analogous to properties of sound, should all admit of being accounted for by mathematical deductions relative to the motions and condensations of a perfect fluid the pressure of which varies as its density. Unless this be the case the argument fails altogether. I consider that by giving a legitimate extension to the principles of Hydrodynamics I have succeeded in satisfying this demand. The character and the grounds of this extension, which consists mainly in adding a third general equation to the two previously recognized, have already been stated in the previous part of this Essay (Arts. 18 and 19), and it will now suffice to mention the principal results which have been thereby obtained.

41. By employing the three general equations it may be proved that, consistently with the necessary relations of space, time, matter, and force, and antecedently to any consideration of the particular mode in which the fluid is put in motion, vibratory motions can take place simultaneously along and transversely to an *axis.* The analysis shews also that each kind of vibration obeys the law expressed by the sine of a circular arc, which law is usually *assumed* in theoretical calculations relating to phenomena of light. [As far as regards direct vibrations these inferences are applicable to motions of the air.] Since the equations which give by integration

these results are linear with constant coefficients, an unlimited number of such vibrations may coexist. It may hence be inferred that vibratory motion produced by any *arbitrary* disturbance must be regarded as consisting of the above-mentioned vibrations, which for distinction may be called *primary*. Moreover, experience seems conclusively to shew, in accordance with what might have been anticipated from the theory, that a disturbance *not vibratory* is capable of giving rise to coexistent primary vibrations of various degrees of breadth, and that *white* light is the result. This fact, and the proposed explanation of it, are deserving, I think, of consideration as constituting an important part of the undulatory theory of light. It is on this principle that the *transmutability* of rays is accounted for, the disturbance, or breaking up, of a series of waves of given breadth being equivalent to a new disturbance, which may be supposed to produce, as above stated, new sets of waves of different breadths.

42. If the *colour* of a parcel of light be not due to the particular vibratory character of the disturbance, it may result from a non-vibratory disturbance, when, after the production, as before mentioned, of waves of different degrees of breadth, some of these waves have been extinguished, or dissipated, by a new disturbance.

43. Similarly, in rays, as originally produced, the transverse motion is alike in all directions from the axis, and the result is *common* light as distinguished from *polarized* light. The mathematical theory shews that the transverse motion and condensation of a ray of common light may suffer, by disturbances, just such modifications as correspond to all the observed laws and kinds of polarized light. It accounts, for instance, fully for the differences between the qualities of bifurcated rays according as the original ray is one of common light or of plane-polarized light.

44. The limited lateral divergence of rays is a simple consequence of a law indicated by the mathematical theory, according to which the transverse velocity and condensation diminish rapidly with distance from the axis. The precise law of lateral diminution of the condensation and motion in the case of composite vibrations has not yet been mathematically determined. The theory, how-

ever, is not inconsistent with the principles on which problems of diffraction are usually treated.

It may, I think, be said that the explanations which the theory is capable of giving of the many and varied phenomena of the class which has been under consideration are primâ facie evidence of its truth.

45. The phenomena of the second class, which depend on relations of the motions of the æther to the constituents of the grosser bodies, do not in the same degree give the means of testing the general theory on account of our being unacquainted with the exact conditions of the interior constitutions of such bodies, and being, consequently, unfurnished with the precise data of the problems whose solutions are required in the theories of this class of phenomena. These problems have reference, for the most part, to the dynamical causes of the Reflection, Refraction, Dispersion, Transmutation, Polarization, and Double Refraction of Light, and necessarily involve hypothetical considerations respecting the ultimate constituency of the substances by the intervention of which these phenomena are produced. The system of physics I am advocating allows of making no other hypotheses respecting the ultimate parts of bodies than that they are discrete atoms, spherical in form, of invariable magnitude, and inert, and that they are held in positions of equilibrium by attractive and repulsive forces which are only modes of action of the æther. It is allowable, besides, for the solutions of certain of the problems, to argue on the supposition that the atoms of the substance have an isotropic, or crystalline, or other definite *arrangement;* but the system admits of no such supposition relative to the *æther.*

46. On the principles above stated, I have attempted (in the work already referred to) to give solutions of many of the Problems of the second class in the Undulatory Theory of Light; and although I have no ground for saying that these difficult problems have all been successfully solved, I may still assert that the adopted principles have sufficed for accounting for two facts which, as far as I know, had not previously received a theoretical explanation: (1) the tangent of the angle of complete polarization

by reflection is equal to the refractive index of the reflecting substance;. (2) one of the rays of a doubly refracting medium, if propagated in a principal plane, is subject to the ordinary law of refraction. The theory also decides without ambiguity, as had not before been done, that the transverse vibrations of a polarized ray are *perpendicular* to the plane of polarization.

HEAT. 47. According to the foregoing theory, the sensation of Light is to be attributed to ray-vibrations of the æther, which by their dynamical action on parts of the eye excite in them vibrations which are propagated along nerves to the sensorium. The effect produced is *vibratory* motion of the atoms of the eye, exclusive of permanent motion of translation, and is experimentally shewn to be due to the *transverse* vibrations of the ray. We are, therefore, at liberty to draw the inference that the force of heat is dynamical action of the *direct* vibrations of rays, and that they produce not only vibratory motions of the atoms of bodies, but also motions of translation, inasmuch as it is known from experience that heat is capable of permanently altering the relative positions of parts of bodies. In order, however, to establish this view it is required to solve the problem of the motion of a very small sphere submitted to the action of a series of ætherial undulations, and to extend the solution to small terms of the *second* order. To this important but difficult problem I have devoted much attention in *The Principles of Mathematics and Physics* under the head of "The Theory of Heat"; and since the publication of that work I have prosecuted the same enquiry, employing for the solution of the problem a process of reasoning differing considerably in its details from that which is there adopted. I shall now endeavour to explain the argument of this process in as intelligible a manner as may be possible without having recourse to analytical symbols for conducting the reasoning.

48. Let us suppose, for the sake of simplicity, the series of harmonic waves to be incident on a *fixed* sphere, and to be composite plane-waves, that is, to be compounded of the primary vibrations (described in Art. 41) in such manner that the transverse vibrations neutralize each other. What under these circum-

stances will be the law of distribution of the condensation on the surface of the sphere, terms of the second order being taken into account?

49. The solution of this problem to the first approximation is exactly analogous to Poisson's solution of the problem of the simultaneous movements of a ball-pendulum and the surrounding air, the two problems requiring for their solutions very nearly the same analytical processes. In the case of the latter, the condensation produced at the surface by the *impulse* of the sphere, and in the other case the condensation produced by its *reaction*, are each found to be *periodic*, and such that at each point of the surface there is on the whole just as much condensation as rarefaction. Consequently, to the first approximation, the impact of the waves tends to produce only *vibratory* motion of the sphere.

50. In solving the problem to the second approximation the plane-waves are still to be regarded as composed of the primary vibrations, provided that in the relation between the velocity and condensation of the latter, terms of the second order be included. By analytical reasoning it may be shewn that the motion in waves so compounded is altogether vibratory, as might, in fact, have been concluded from the consideration, that if it were not so there would be a non-circulating flow of the æther in a given direction, which is hydrodynamically impossible. Now as these compound waves are propagated with a uniform velocity, there exists, as is known, a necessary relation between the velocity and the condensation of the vibratory particles[1].

51. Taking account of that relation, and supposing the velocity to be expressed by a harmonic function, I employed the two general hydrodynamical equations given by the principle of constancy of mass and D' Alembert's Principle to calculate to the second approximation the condensation at any time at all points of the surface of the fixed sphere. The calculation, which was

[1] See the proof of Proposition IX. in pp. 190—192 of the *Principles of Mathematics and Physics.* If V be the velocity, σ the condensation, and a the rate of propagation, the relation between V and σ to terms of the second order is expressed by the equation $\sigma = \frac{V}{a} + \frac{V^2}{a^2}$.

long but otherwise not difficult, gave the result that the sum of the accelerative forces acting on the sphere in the positive direction is equal to the sum of those acting in the opposite direction so that there is no residual *accelerative* action. It appeared also that if the sphere were free to move, the accelerative forces would be altered to a small amount in a constant ratio by the resistance of the fluid to its motion, and that in this case also there would be no resulting acceleration of the sphere in either the positive or the negative direction.

52. It must, however, here be said that this method of solving the problem appears to be defective in principle, because it does not adequately take account of the *transverse* action brought into play when the composite-waves are disrupted by incidence on the sphere. The calculation of the law and amount of the lateral action in this case is attended with the same kind of difficulty as that met with in the solution of the problem of *diffraction* in the Undulatory Theory of Light. I think, however, that, without directly overcoming the mathematical difficulty, the following argument will conduct to results by which the purpose of the present investigation will be sufficiently answered.

53. The above-mentioned process does not explicitly take account of the circumstances of the motion which are attributable, after the disturbance of the plane-waves by incidence on the atom, to these waves *being compounded of the primary vibrations.* The laws of the primary vibrations are ascertainable antecedently to any supposition respecting the mode of disturbing the fluid[1], and must on that account be regarded as resulting exclusively from the *elasticity and inertia* of the fluid. In order, therefore, in any proposed case of disturbance, to take into account the inertia of the fluid (which it is absolutely necessary to do), the actual motion must be assumed to be composed of the primary motions, and the consequences of such composition under the given circumstances must be investigated. It is true that in the before-mentioned process the plane-waves were supposed to be

[1] See Propositions XI—XV in pp. 201—228 of the *Principles of Mathematics and Physics.*

composed of primary vibrations, and terms of the second order in the expressions for the latter were included; but what takes place in consequence of that composition when waves so compounded are disturbed, or broken, as they would be in the case before us of incidence on a fixed sphere, was not ascertained.

54. Now it is evident that the condensation at the surface of the sphere would be modified by the circumstance, that, since the incident waves consist of components having both direct and transverse vibrations, *lateral* action is brought into operation by their incidence on the sphere. The kind of effect thence resulting may be conceived of as follows. If undulations of very small breadth were incident on a very large sphere, the condensations which reach the farther half of the surface would be much diminished by reason of limited lateral divergence on being transmitted beyond the first half. In extreme cases a portion of the fluid in contact with that part of the hemispherical surface which is farthest from the part of the other on which the waves are immediately incident, might be altogether undisturbed. On the other hand, if waves of large breadth were incident on a very small sphere the condensations might become by lateral divergence greater on the second half surface than on the first, because the conditions of the motion would approximate to those of the *primary* direct and transverse vibrations relative to an axis, in which the condensation corresponding to a given velocity is greater than in plane-composite waves in the ratio of the square root of the apparent, to the square root of the actual elasticity[1]. Accordingly, so far as the action of the primary component vibrations is effective, the condensation, or rarefaction, on the surface might either *decrease* or *increase* as the abscissa reckoned from the origin of the waves is greater. In the actual physical circumstances of the æther and the atoms the gradation must be extremely small; otherwise, by reason of the vast elastic force of the æther, the accelerations of the atoms would exceed the amounts ordinarily indicated by experiment and observation, the accelerations in explosive mixtures, and those which the atoms of the

[1] See *Principles of Mathematics and Physics*, pp. 222 and 223.

comæ of certain comets must undergo in the neighbourhood of the sun, being extreme cases.

55. Hence the distribution of the condensation at the surface of a fixed sphere on which a series of harmonic waves is incident, is determined by *two* circumstances; (1) the variation due immediately to the variation, at a given time, of the condensation of the incident wave in the direction parallel to that of the incidence, together with the small variation due to the reaction of the sphere; (2) the variation ascribable, as explained in the preceding article, to lateral action resulting from the fluid's inertia. The accelerative action on the sphere produced by the former superficial variation of the condensation will be wholly *periodic*, being, in fact, the same as that obtained by the process mentioned in Art. 51. Hence any force tending to produce a permanent motion of translation of the sphere must be referable to the variation (2). That this variation of the superficial pressure gives rise to a residual acceleration of the sphere in either the positive or negative direction may be shewn as follows.

56. Considering by itself, for the reason just given, the variation of the condensation due to the lateral action, if we put σ for the superficial condensation at any time in the plane passing transversely through the centre of the sphere, the condensation at any point of the surface will be equal to the product of σ and a constant depending only on the position of the point. This follows from the character of the transverse vibrations, which is such that their composition produces condensations following the same law as the maximum condensations due to the composition of the direct vibrations, differing only in amount. Hence the whole accelerative action on the sphere at the given time will vary as σ. Now as σ may be taken generally to be the *mean* superficial condensation, and proportional to, if not identical with, the condensation due to the incident wave, for every positive condensation σ there will be a negative condensation σ' such that the velocity corresponding to the latter is just equal and opposite to that corresponding to the other (see Art. 50). Hence as far as depends on these condensations the accelerative action on the sphere varies as

$\sigma + \sigma'$. But by the formula in the note to Art. 50, $\sigma + \sigma'$ varies as the square of the velocity (V^2). Consequently the excess of force tending to accelerate the sphere may be taken to vary *at any instant* as the square of the velocity in the part of the wave which is coincident as to position with the centre of the sphere. But the square of the velocity in a harmonic wave is partly constant and partly periodic. Hence, leaving out of account the periodic part, the sphere will be urged by a constant accelerative force, which will be in the same direction as that of the incidence of the waves, if the lateral action produce a *decrement* of condensation (see Art. 54), and in the contrary direction if it produce an *increment.* The former case corresponds to a *repulsion*, and the latter to an *attraction.* Supposing the waves to emanate from a centre, since in that case the velocity varies inversely as the distance from the centre, the force will *vary inversely as the square of the distance from the centre*[1].

57. The *attraction* of bodies by the intervention of vibrations of the *air* has actually been recognized by experiment. (See an Article on the experimental demonstration of this fact in the Number of the *Philosophical Magazine* for November 1870, and the theoretical explanation of it which I have proposed in the Number for April, 1871.) The air can hardly be susceptible, considering its density and elasticity, of such minute and rapid vibrations as would be required to produce, by means of lateral

[1] According to the views expounded in Arts. 51—56, the accelerative action on the surface of the fixed sphere might be expressed by the formula $H\sigma + K\dfrac{d\sigma}{dx}$, σ having the same signification as in Art. 56, and H and K being certain constants depending generally on the breadths and intensities of the waves, and the dimensions of the atoms. Also taking σ' in the same relation to σ as in Art. 56, the accelerative force due to negative condensation would be $H\sigma' + K\dfrac{d\sigma'}{dx}$, so that the residual force is expressed by $H(\sigma+\sigma') + K\left(\dfrac{d\sigma}{dx} + \dfrac{d\sigma'}{dx}\right)$. But since $\sigma+\sigma'$ varies as V^2 (see note to Art. 50), the second term of that expression varies as $V\dfrac{dV}{dx}$, and the force it represents is therefore wholly periodic. This is in accordance with the second term of the expression for the accelerative force in p. 452 of *The Principles of Physics.*

action, a sensible amount of continuous *repulsive* action on solid bodies; and, accordingly, no such action has hitherto been detected by experiment.

58. The foregoing argument is nearly the same as that which is conducted by analytical calculation in a communication to the *Philosophical Magazine* for September 1872. I take occasion to add that I consider the present reasoning to be much more satisfactory than that which is concluded in page 452 of *The Principles of Physics*, which, notwithstanding what I say in pages 453—455, fails to give a theory of attractive and repulsive forces from not explicitly taking account of lateral action. The formula for the accelerative force in p. 452 may, however, be adduced to prove that the acceleration, so far as it depends on the variation of condensation of the incident waves, would only be changed in a constant ratio, if the sphere were not fixed.

59. The consequences, as regards the part of the accelerative force depending on lateral action, of supposing the sphere to be moveable, may be inferred as follows. Conceive the velocity generated by that force to be impressed at each instant on the sphere and on *all parts* of the fluid, so that the sphere will remain at rest, and the original condensation of the incident waves will in no respect be altered; only a new condensation will be generated by the impact of the fluid on the sphere consequent upon the impression of velocity. In the case of a very minute sphere this condensation must be excessively small; and the lateral action to which its *periodic part* may give rise, if it be appreciable, may be considered to be combined with the lateral action due to the original waves. But the *non-periodic* part of the sphere's motion, produced, as above shewn, by the constantly accelerating action of the waves, will give rise to a resistance of the fluid which may become sensible in cases of great acceleration. For instance, Encke's comet may on this account suffer retardation in the parts of its orbit where the velocity changes fastest.

60. The mathematical determination of the condensation and motion resulting from the incidence of plane-waves on a small fixed sphere, shews that if the sphere be extremely small there

is no perceptible reflection of the incident waves, or diversion of their propagation into a different direction, but that the reaction of the sphere gives rise to a *new* condensation of very small amount, which is propagated in all directions along the radii produced, and varies in intensity according to the cosine of the angle which the direction of the propagation makes with the direction of incidence. These *derivative* waves, as they may be called, may be of very various breadths, depending on the breadths of the incident waves; but the maximum of velocity or of condensation in each will be excessively small, and so much the smaller as the breadth of the incident undulation from which it is derived is greater. Conceiving, now, the sphere to be a constituent atom of any mass, and waves to be incident on it from an immense number of surrounding atoms of the mass, as also from the atoms of separate adjacent masses, we may assert that under the condition of equilibrium of the atom, the condensation resulting from the composition of all the derivative waves is the same at all points of its surface, and at any distance from the surface is a function of the distance from its centre. Also it may be presumed that this condensation of the compound spherical waves is still so small that when they are incident from any given atom on an adjacent atom the consequent condensation at the surface of the latter *decreases*, by reason of lateral action, in the direction of the incidence. Accordingly this atom will be *repelled* by the other, and so much the more as the distance between them is less. No two atoms can ever come into collision. This, according to the adopted hypotheses, is the mathematical theory of *atomic repulsion* which I consider to be identical with the repulsion of heat.

61. Respecting the theory of *radiant heat* it will suffice to make the following few remarks. It may be inferred from experiment that there are undulations of the æther of considerably greater breadth than those which acting on the parts of the eye produce in us the sensation of light. These, like the luminous rays, are susceptible of reflection, refraction, dispersion, &c. When a substance is *diathermous*, that is, when heat-rays pass through it in the same manner as light-rays pass through trans-

parent substances, little or no effect is produced by the derivative waves due to the reaction of the atoms, because they either coalesce with the original waves or are mutually destructive. Since also there is no perceptible reflection of the original waves from the atoms, the substance receives no perceptible heat. But if it is not diathermous, the waves are incapable of undisturbed propagation, and are consequently broken up into parts of every variety of breadth, which, being scattered in different directions, give rise by their incidence on the atoms to derivative waves proper for producing atomic repulsion. Thus the substance is *heated.* The same effect, only in less degree, must result from the entrance of luminous waves into an *opaque* substance, inasmuch as experience shews that rays of light possess generally the power of heating.

62. The foregoing theory of atomic repulsion being admitted, we may proceed as follows to account for the existence of a counteracting attraction. Let us suppose that a spherical portion of a homogeneous substance is filled with an immense number of discrete atoms, and that each of them is the centre of undulations such as those which, as explained above, act dynamically as atomic repulsion. The composition of all these sets of undulations may be supposed capable of generating within the spherical portion a resultant series of undulations the velocity and condensation in which will *mask* those of the components, and be very approximately functions of the distance from the centre of the spherical space. As the component motions are all vibratory, the motion will also be vibratory in the compound waves, and when the number of atoms is extremely large the breadth and intensity of these waves may be supposed to exceed very much the breadth and intensity of any of the sets of component waves. In fact, since the number of atoms in a given spherical space varies as the cube of its radius, whilst the condensation of the waves emanating from each atom varies as the simple inverse of the distance, it follows that the compound condensation *increases* with the distance from the centre of the space. The case is analogous to the increment of gravity with the distance from the earth's centre, excepting that, instead of increasing simply as the distance, this condensation will increase more

nearly as the *square* of the distance. Now it is evident that under these circumstances we may suppose that the compound waves, on increasing the magnitude of the spherical space, become eventually, as to breadth and intensity, of the order of those which, as already explained, produce at the surface of an atom on which they are incident a condensation *increasing* in the direction of propagation. These waves will then act as *attractive* forces tending to the centre of the space, and may be conceived to be just adequate to counteract the tendency which the atomic repulsions have to separate the atoms and cause expansion. The congeries of atoms in the spherical space would thus constitute a body which is *self-containing*. The aggregate of the least number of atoms of a given substance which suffices for constituting such a body I propose to call a *molecule*, or mass of atoms, and the attraction whereby its status is maintained, *molecular attraction*. Notwithstanding that a molecule contains an immense number of atoms, it must yet be conceived of as so extremely small in all fluid and solid bodies as not to admit of being distinguished or measured by any means at present known.

The preceding discussion may serve to shew in what manner an explanation of the nature of heat is derivable from our original hypotheses respecting the æther and atoms. The purpose of this Essay does not require more to be said on the theory of heat.

GRAVITY. 63. The reason for treating of the force of gravity next in order is, that like the forces of Light and Heat it results from *vibratory* motions of the æther, and in that respect differs, as will subsequently appear, from the forces of Electricity, Magnetism and Galvanism. The discussion in Arts. 48—59 of the modes in which vibratory and translatory motions of a spherical atom are produced by the action upon it of a series of ætherial vibrations, embraces all cases in which either attraction or repulsion results from such action, and is, consequently, inclusive of the attraction of gravity. Since gravity is known from observation to be always an *attractive* force, it must, according to the theory explained in Art. 54, be referable to the action of a class of vibrations of such breadth and intensity that, when they are

incident on an atom, the consequent condensations and rarefactions that are due to the inertia of the fluid in motion, *increase* on the surface of the atom in the direction of incidence. It will, therefore, be proper to enquire now in what way vibrations of this class are generated.

64. This enquiry has been already initiated by what is said in Arts. 60 and 62 on the theories of *atomic repulsion* and *molecular attraction.* According to those theories the ætherial vibrations to which molecular attraction is to be attributed are *composed* of the vibrations of atomic repulsion which emanate from all the atoms constituting a *molecule,* the composition producing an order of vibrations the breadths and intensities of which fulfil the conditions required for acting on atoms attractively. Let us, therefore, now suppose that the attraction-vibrations become, by transmission to distances from the centre of the molecule which are large multiples of its radius, of extremely small intensity, or are actually resolved by the divergence of the prolonged radii into the original components. In either case they would again act *repulsively,* and for distinction this action might be called repulsion of the *second order.* If then we take a spherical space equal in extent to a very large multiple of the space containing the atoms of a single spherical molecule (as defined in Art. 62), the repulsion-waves of the second order emanating from the molecules within this space, which together constitute *a molecule of the second order,* might by composition, as in the first instance, produce *a second order of attraction waves,* whereby the repulsion of the second order would be controlled. By proceeding to composite waves of the *third order* emanating from the second-order molecular components of a molecule of the *third order,* we should probably reach those which account for the attraction of *gravity.* Even these third-order molecules can only be regarded as elementary parts of the attracting bodies; for otherwise no definite results could have been obtained from experiments made with the comparatively small masses used by Cavendish, Reich, and Baily for determining the mean density of the earth.

65. It does not appear that the *breadths* of the gravity-waves

can be determined precisely by any known experimental data; but as they must be supposed to traverse dense solids and fluids without undergoing sensible retardation, or being refracted at the surfaces, and as the retardations of the propagation of waves in media are, *cœteris paribus*, in some inverse proportion to the breadths, it follows that the breadths of gravity-waves must be very considerable compared with the breadths of the waves of radiant heat (see Art. 61), or even with those of the electricity-radiants which will shortly come under consideration. It is not, however, certain that they suffer no sensible amount of retardation or refraction, inasmuch as certain *local irregularities* of gravity have been noticed, which it seems hard to account for except on the hypothesis of some degree of refraction of gravity-waves. (See what is said on this point in *The Principles of Physics*, p. 502.)

66. Although, according to these views, the force of gravity is referable to the action of waves which take time in passing from point to point of space, this circumstance affects in no respect the amount of the force, so long as the rate of propagation is not sensibly different within substances from what it is in free space. For in that case the translatory action of the waves is constantly the same at the same distance from their origin, and therefore the same as if they were transmitted *instantaneously*. That the force of gravity is apparently so transmitted has been inferred from facts of observation.

67. From what is argued in Art. 56, the acceleration of an atom by the gravity-waves is expressible by the formula $H\sigma$, σ being the condensation of the æther in the transverse plane passing through the centre of the atom, and H a constant factor, depending generally on the breadth and intensity of the wave and the magnitude of the atom. To complete the theory of gravitating force it is necessary to ascertain exactly the form and value of this factor. But in the existing state of the science of Hydrodynamics it does not appear possible to do this, on account of our not knowing the proper method of treating hydrodynamical problems which involve lateral action and the production thereby of limited lateral divergence. There is, it is true, the method of dividing the front

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of a wave into small elements, adopted by Fresnel in his theory of diffraction, the mode of applying which is, I think, justified by the law of the composition of waves which I have deduced from the principles of Hydrodynamics (see Art. 41), namely, that the component vibrations are partly direct and partly transversal relative to axes. I have not, however, made use of that method, not having been able to convince myself that the mathematical reasoning usually employed in its application is strictly founded on hydrodynamical principles. Until the analytical expression for the factor H be obtained, it will not be possible to determine whether the fact that all bodies are equally attracted by gravity is due to their constituent atoms being all of the same magnitude, or to the moving force of each being proportional to its magnitude. If the former hypothesis be adopted, the difference between one simple body and another will depend only on the *arrangement* of the atoms. The discovery by chemical analysis that the atomic weights of such bodies are very nearly in the proportion of simple numbers, as 1, 6, 8, 14, &c., gives support to this hypothesis.

68. Assuming that the stars are large masses like the Sun, and that they act on each other only by the attraction of gravity, it does not seem possible to conceive how the whole stellar system, inclusive of the Milky Way, or how a resolvable cluster of stars, can be in stable equilibrium. To ensure stability there must be some counteracting force. The present theory, which professes to account for the *modus operandi* of all physical force, might reasonably be objected to if it failed to explain by what agency the stability of the system of the Universe is maintained. This, however, I conceive may be done by merely extending the application of the principles on which the state of equilibrium of the component atoms of any substance was shewn to be the result of the combined action of atomic repulsion and molecular attraction. It was indicated in Art. 64 that these forces of the *first order* might coexist with repulsions and attractions of the *second* and *third* orders, and that corresponding to the three orders of attractions there would be three orders of molecules. It was also considered probable that the attraction-waves of the third order

emanating from the third-order molecules accounted for the force of *gravity;* but the limitation to three orders is not necessary. It may, at least, be presumed that the totality of the waves from large masses like the planets, or the Sun, would constitute attraction-waves of such intensity that they would continue to be attraction-waves after propagation to very great distances, and to attract according to the law of the inverse square. But considering the vast distance of any star from those nearest to it, it is conceivable that within that interval the attraction-waves might become repulsion-waves by diminution of intensity with the distance from their origin, the breadths remaining the same. In that case they would be capable of transmission through large masses without suffering disruption or retardation, and of repelling *bodily* each of the surrounding masses. According to these views there would be a general law of *mutual repulsion between neighbouring stars.* This repulsion might then be counteracted by another order of gravity-waves formed by the composition of waves from all the individuals of the whole stellar system. The stability of the heavens would thus be effected by forces acting analogously to the combined action of the atomic and molecular forces which hold the atoms of bodies in equilibrium. The conjoint action of the cosmical repulsions and attractions might, however, consistently with this theory produce *periodic* motions of the stars about mean positions, and the theory would receive much confirmation if the existence of vibratory proper motions of the stars should be established by astronomical observation.

69. It may be farther remarked that the above-considered repulsion-waves would cease to act repulsively *on masses* after being propagated to distances from their origin at which they undergo resolution by the mere effect of the divergence of the axes of their components. Thereafter they would partly contribute to the maintenance of the internal heat of the stellar masses by being incident upon them in the form of heat-radiants, and partly go to form that order of attraction-waves which, as stated above, together with the repulsion-waves, equilibrate the whole system of stars.

ELECTRICITY. 70. From the antecedent theory of atomic repulsion and molecular attraction it may be inferred that any

atom in the *interior* of a fluid or solid body is in equilibrium, as far as regards the action of these forces, because the body may be considered homogeneous throughout the small extent within which are included the atoms whose action on the given atom determines its position, so that the resultant of their action along any straight line drawn through its centre is zero. But the case of an atom at or near the *boundary* of the fluid or solid substance will be different. Here the equilibrium of an atom cannot subsist unless there be a gradual *decrement* of the density of the substance towards its boundary within a superficial stratum of small but definite thickness. In that case the resultant of the atomic repulsions on an atom at the boundary will be diminished by reason of the diminution of the number of atoms in the adjacent space, while the resultant of the molecular attractions will not be diminished in the same degree, because the sphere of their activity is much larger than that of the atomic repulsions. Under such conditions the molecular attraction tending *inwards* may at the boundary just counteract the atomic repulsions tending *outwards*. The gradation of density, and equilibrium of the atoms, in the superficial stratum, would be maintained by the combined action of the attractive and repulsive forces in a manner analogous to the maintenance of the gradation of density of the earth's atmosphere by the simultaneous action of the earth's gravity and the expansive force of air. Also it would follow from these conditions that the molecular attraction may be of sensible amount within a certain small distance outside the boundary. This, in fact, is the explanation, according to the present theory, of *capillary attraction.*

71. But it is evident that the state of equilibrium of the superficial atoms might be disturbed by the application of an extraneous force; and from the theory it might be anticipated that an effect of this kind would result from *rubbing* the surfaces of two substances together. In fact, by such means phenomena are produced which have been called *electric* because they appear to have first attracted notice on rubbing a piece of amber with silk. It has also been ascertained by experiment that *two* kinds

of electricity are developed by friction, and that one kind attaches itself to one of the substances rubbed and an opposite kind to the other. The explanation of this fact by the proposed theory is such as follows.

72. The mechanical operation of rubbing disturbs the superficial atoms in such manner that in one of the substances they are *pushed in*, and in the other they are *drawn out*, beyond their ordinary neutral positions. That these two effects must be simultaneously produced will appear from the consideration that the applied forces operate to displace the atoms by the intervention of the *mechanically equivalent* atomic and molecular forces of the two substances, and the resultants of these, where the substances are in contact, necessarily act on both in the *same* directions relative to the common tangent planes at the points of contact, thus compressing one and dilating the other. The two kinds of electricity are called *vitreous* and *resinous*, or *positive* and *negative*. I assume that the *vitreous* or *positive* electricity corresponds to the *compressed* state of the superficial atoms.

73. It is, however, to be considered that very small changes in the distances of the superficial atoms from each other may cause very large changes in the atomic repulsions, without sensibly affecting the molecular attractions, the sphere of activity of the latter being so much larger than that of the repulsions. Consequently a change of the atomic repulsion, either by compression or dilatation of the superficial atoms, will not, in general, be counteracted by a simultaneous change of the molecular attraction; and we have, therefore, to consider, next, what forces maintain that state of equilibrium of the superficial atoms which, as is indicated by electrical experiments, exists with more or less permanence after the disturbance.

74. I shall endeavour to shew that the electrified state of a substance is maintained by the action of those repulsions and attractions of the *second order* which are treated of in Art. 64. In whatever way they act it is plainly a necessary condition that every atom should be acted upon so as to be in equilibrium, whether it be in the interior of the substance, or in the extreme

superficial stratum. For the purpose of obtaining a precise conception of the mode in which that condition is satisfied under the action of the forces both of the first and second order, let us take the simple case of an electrified sphere, and suppose it to be electrified *positively*, or that the atoms in the superficial stratum are *closer together* than they are in the neutral state. In this case the atomic repulsion outwards of the extreme superficial atoms will be *greater* than that which results when they are in their undisturbed positions. To account for the counteraction of this effect it is only necessary to suppose that the disturbance causes the density of the sphere to vary by a gradual small *increment* extending from the centre to the surface, the mass of the sphere remaining the same. For under these circumstances it may, first, be assumed that at each point in the *interior* an excess of first-order atomic repulsion towards the centre above first-order molecular attraction towards the surface is just equal to an excess of second-order molecular attraction towards the surface above second-order repulsion towards the centre, the latter excess being due to the assumed *accumulation* of matter towards the surface. In fact, this equality ensures that each atom is in equilibrium under the action of the four forces, and it is precisely the satisfying of this condition that determines the *rate* of increment of the density. Again, it may be assumed, with respect to any atom in the *superficial stratum*, that the excess of atomic repulsion outwards, due to the compression caused by the disturbance, is just counteracted by the excess of molecular attraction inwards, due to the above-mentioned accumulation of matter towards the surface. Hence all the atoms of the substance would in this way be in equilibrium.

75. If the sphere were *negatively* electrified, that is, if the superficial atoms were *farther apart* than in their ordinary state, the same argument, *mutatis mutandis*, would shew that the condition of the equilibrium of all the atoms might be satisfied by a small extraordinary *decrement* of density from the centre to the surface.

76. Other considerations have to be entered into if it be

required to find the conditions of electric equilibrium of any substance differing in form from that of a sphere. And, first, a distinction is to be made between *conductors* and *non-conductors.* The facts signified by these terms may be supposed to depend on modes of action of the atomic and molecular forces resulting from the particular constitutions of the substances as to the magnitudes and arrangement of their constituent atoms. But it is no part of the present theory to enquire what these constitutions might be, this being one of a class of problems the solution of which can hardly be attempted without a previous knowledge of the nature and laws of the physical forces, such as that which it is the express object of this Essay to shew how to obtain. It will suffice to state that the distinction, according to this theory, between conductors and non-conductors is, that in the former a disturbance of the atoms in the superficial stratum is spread, as soon as it is generated, over the whole of the surface of the body by *propagation through the stratum,* while in the case of a non-conductor the disturbance is propagated from the place of excitement either to a very limited extent, or very slowly.

77. Consequently in considering the electric state of a perfect conductor *three* kinds of force have to be taken into account: (1) the repulsions and attractions of the first order, maintaining the variation of density in the superficial stratum; (2) the transverse force at any point of the stratum, to which is due the tendency of the electricity to spread superficially; which force, when the electricity is in a statical condition, is proportional to the decrement of electricity (as measured by an electrometer) in a small given interval on the superficies, and is in the direction of the decrement; (3) the repulsions and attractions of the second order, pertaining to the molecules of the second order, the resultant attraction of each such molecule acting on all surrounding atoms according to the law of the inverse square. The equilibrated action of these forces determines the variation of density, both in the superficial stratum and in the interior of the body, and, consequently, the physical conditions of the body's statical electricity. The intensity of the electricity at any point of the

surface, whether positive or negative, is proportional to the deviation of the variation of the density in the superficial stratum, estimated normally, from that amount of variation which pertains to the neutral state.

78. Suppose, for instance, the body to be a cylinder like the conductor of an electrical machine, with rounded ends, and symmetrical about a middle transverse section, and in its neutral state let its density, excepting within the superficial stratum, be the same throughout. Then if it be electrified *positively*, it is easy to see without employing symbolical calculation, that the action of the above-mentioned forces will produce an *accumulation* of the electricity towards the two ends, and a minimum of electricity at its middle section, and that the disturbed state of the superficial variation of density will be accompanied by an increment of interior density towards each end. If the cylinder were charged with *negative* electricity, there would be an accumulation of negative electricity towards each end, a minimum of electricity at the middle section, and at this section the interior density would be a maximum. These results as to the distribution of the electricity accord with experience. Other illustrations of the same kind might be added; but, perhaps, enough has now been said to shew what, according to the adopted hypotheses, is the physical condition of an electrified body.

79. It remains to account for the *induction* of electricity in a neutral body by one that is electrified, and for *the mutual attractions and repulsions* of electrified bodies. The proposed theory offers the following explanations of these facts.

First, it is to be noticed that while the action of first-order attractions extends sensibly beyond the boundaries of fluid and solid substances only to such distances as suffice to account for the phenomena of capillary attraction, we may suppose the action of the second-order *attractions* to be effective at much larger distances, although the magnitude of the second-order molecules, and the sphere of their *repulsive* activity, are still very small. Now on this supposition it must be admitted that this class of attractions is operative whether the substance from which they

emanate be electrified or not, and it is, therefore, necessary to explain why no effect is perceptible if the substance be in a neutral state. This, I conceive, may be done by supposing that in the action of this class of vibrations there is between different substances the same kind of *reciprocity* as that whereby their *temperatures* are equalized by means of radiant heat. The neutralization of the electricities of different adjacent bodies may be conceived to be effected by second-order attraction-waves emanating from all points of each and incident on all the other bodies, as the equality of the temperature of a group of bodies results from the emanation of waves of radiant heat in all directions from each of them; and as this equality of temperature is disturbed under certain conditions, such as those of the well-known experiment which exhibits the reflection of *cold*, so the neutral state of a body's electricity is disturbed by friction or other means, in consequence of which the ordinary reciprocal action of its second-order attractions and those of surrounding bodies no longer subsists, and free electricities are developed. In this way radiant waves of free electricity, of a different order from those of radiant heat, become operative, and produce effects the character of which will be made apparent by the explanation I now proceed to give of the induction of electricity.

80. As soon as a body is electrified, the reciprocal action of the second-order attraction-waves between it and adjacent bodies is disturbed by reason of the change of the mode and degree of its attraction, consequent upon the simultaneous induction of interior gradation of its density; and whether the change be in excess or defect, the disturbance of the reciprocal action must take effect on surrounding bodies. It may, I think, be assumed that the effect is such as follows. From experiment we may conclude that an electrified body acts on substances of all kinds, although in greater degree on conductors than on non-conductors. Let us, therefore, suppose the second-order attraction-waves to be capable of traversing all substances without undergoing disruption or dispersion. In this respect they would differ from the waves of radiant heat, which, as we have seen, can be

propagated without disruption only in certain substances which have the property of being diathermous. Neither can they be of the same order or kind as waves to which might be attributed the force of gravity; for these must be independent of local disturbances such as those produced by friction, and be capable not only of traversing, without being broken up, all substances alike, but also of acting, by reason of their greater breadth, on all the atoms of a given substance so as to tend in no sensible degree to alter permanently their relative positions; whereas the waves emanating from an electrified body must be supposed to be capable of changing the relative positions of the atoms of any adjacent body, otherwise there would be no induction of electricity. These waves, which for distinction might be called electricity-radiants, would, therefore, seem to be intermediate to gravity-waves and the waves of radiant heat. The induction may be conceived to take place in the following manner.

81. Suppose the electrified body to have, as before, the form of the conductor of an electrical machine, and to be electrified *positively*. Then, according to the foregoing argument, if a body in a neutral state be made to approach the rounded end, it will be acted upon by second-order attraction-waves emanating chiefly from parts in the neighbourhood of that end, and will be traversed freely through its whole interior by these waves. Also since, by hypothesis, the conductor is positively electrified, this attractive action, according to explanations given in Art. 79, will *exceed* that of the same body in its neutral state, and the difference will be a disturbing attraction acting on all the atoms of the neutral body. Let us assume, at first, that the body is *fixed*, and, for the sake of distinctness, that it is a conductor of the same form as the first, and having its axis in the same direction. Then all its atoms will be in positions of equilibrium by the combined action of its proper attractive and repulsive forces and the above-mentioned differential attraction of the electrified body. Hence it follows that the atoms in the superficial stratum will be *more* drawn from their ordinary positions by that attraction than the atoms in the interior, inasmuch as their ordinary equilibrium is

less stable than that of the latter. Consequently the superficial atoms of the neutral conductor will be displaced in such manner that those on the side towards the electrified body will be *drawn out*, and those on the opposite side will be *pulled in*. The same attraction will also produce a very gradual *increment of interior density* from the nearer to the more remote parts of the conductor, because, of two atoms separated by a small interval, that which is the nearer to the source of attraction will be drawn from its neutral position more than the other. It is, however, to be observed that the effect thus produced will be very small, and that the increment of interior density in that direction, which, as previously argued, is a necessary condition of the equilibrium of the atoms in their disturbed positions, will be due mainly to the proper repulsive and attractive forces of the neutral body itself, acting in the manner indicated in Arts. 74 and 75 with respect to the equilibrium of the atoms of an electrified body. Thus by the combined action of all the forces the induced electricity is maintained in such manner that the parts of the neutral body towards the inducing body are *negatively* electrified, and the opposite parts *positively* electrified.

82. If, however, the inducing body be *negatively* electrified, the second-order attractive waves incident on the neutral body will be of *less* intensity than those incident upon it under the condition of ordinary reciprocal action of the electricity-radiants, and consequently the *differential* action will relatively be a *repulsion* whereby the atoms on the side towards the inducing body will be *pushed in*, and those on the opposite side *pushed out*. Hence the former will be *positively* electrified, and the other *negatively*. This result and that obtained above account for well-known experimental facts.

83. Further, it is to be remarked that since the induced electric state of the neutral body is maintained in both cases by attraction-waves emanating from the electrified body, if the latter be removed the induced electricity will disappear, and the body return to its previous neutral state. This result also is confirmed by experience.

84. It is an obvious inference from the theory that the induced electricity of the neutral conductor will act reciprocally on the inducing conductor, as is known to be the case from observation.

85. The foregoing results have been obtained on the supposition that the neutral body is fixed. Let us now suppose it to be *moveable.* Then it might be supposed that as the inducing waves are in all cases attractive, and capable by their attraction of altering the relative positions of the atoms of the neutral body, they must also be capable of moving it in some degree *as a whole.* In that case the disturbance of the reciprocal action of the electricity-radiants, consequent upon electrifying one of two neighbouring conductors, would cause the electrified one, if electrified positively, always to attract the other, and if electrified negatively, always to repel it. But experience shews that this is not the law of nature; and accordingly our theory, if well founded, should be capable of indicating wherein this reasoning fails, and of giving a different account of *electrical attractions and repulsions.* To shew how this may be done without adding to or deviating from the original hypotheses in any respect is a critical and essential part of the theory, to which attention must now be specially directed.

86. In the first place it is to be considered that the electricity-radiants which are supposed above to traverse substances without undergoing transmutation, act on the atoms at a given position by a force which is proportional at a given time to the variation at that position of the condensation of the æther in a given small linear space. Now, the maximum condensation of the waves being given, it may readily be shewn that at corresponding positions in different waves this force is inversely proportional to their breadths, and that its variation in a given small space is inversely proportional to the *squares* of the breadths. Let us, therefore, assume the breadth of the electricity-waves to be so small that within the extent of a *second-order* molecule that variation of the force is recognisable by its effects. In that case the electricity-waves would continually alter the relative positions of the atoms of the

molecule by putting them in vibration, in consequence of which a continuous opposite action of *second-order repulsive* waves would be excited. Now if the steady translatory actions of these two opposite forces were *equal*, they would simply have the effect of maintaining the atoms in new mean positions of equilibrium. In fact, it has already been argued (Art. 75 and 76) that the interior gradation of density of the originally electrified body is due to the equilibrated action of second-order attractive and repulsive forces, and it may, accordingly, be presumed that a like effect might be produced by the same order of forces in the inductively electrified body. Also on the supposition of the exact equality of the opposite forces, there would be no residual force to act against a fixed obstacle preventing motion of the body as a whole, and consequently if the obstacle were removed, and the body became, as is now supposed, moveable, no motion would ensue. Thus the attraction-waves of the second order issuing from the inducing body would, according to the foregoing views, be wholly employed, first, in altering the relative positions of the atoms, both superficial and interior, of the inductively electrified body, and then in maintaining them in the new positions.

87. If, however, the repulsive and attractive forces acting on the individual atoms in the superficial stratum and in the interior of the inductively electrified body do not, as supposed above, exactly counteract each other, there would then be a residual attraction whereby the electricity-radiants might be capable of sensibly moving the body as a whole. As this force, varying inversely as the square of the distance from its origin, would attract the nearer atoms in greater degree than those more remote, it would *relatively* displace the superficial atoms of the body in *opposite* directions from the mean positions, and thus induce the *same* electricity on the farther as on the nearer side. As no such effect has been recognised by experiment, we must conclude that this translatory action is either too small to be detected, or that it is masked by the superior efficacy of another kind of attractions and repulsions the theory of which I now proceed to discuss.

88. Let us conceive the æther to be in motion in the interior

of a substance the density of which, as measured by the number of its atoms in a given space, increases very slowly in a given direction, the space occupied by the matter of the atoms being throughout extremely small compared to the intervening space; and for the sake of simplicity let us suppose, at first, that the motion is *steady*. Then the solution of the problem of the incidence of fluid in motion on a small sphere (cited in Art. 49) leads to the inference that the quantity of fluid which at each instant is passing a plane transverse to the mean direction of the motion is not altered by the reaction of the sphere. The result would be the same in the case of incidence on a congeries of atoms, assuming, as above, that they fill a very small portion of a space of given extent containing a very large number of them. But it is evident that under these conditions, if the density of the substance which the atoms constitute increase in a certain direction, the velocity of a *given* small portion of the fluid will be made greater by passing into positions of greater density, on account of the contraction of channel by the greater number of atoms. Hence, taking two positions near each other on a line drawn in the direction of the increment of density, the velocities at these points, supposing that *cæteris paribus* they would be equal, will, in consequence of the variation of density, be to each other in the ratio of the densities at the points. Now by Hydrodynamics it appears that in every case of steady motion, under whatever circumstances it takes place, the pressure is everywhere less as the velocity is greater, the variable part of the expression for the pressure being proportional to the square of the velocity with a negative sign. Hence the aggregate of the atoms in any small given portion of the fluid will act as an extraneous accelerative force always urging the fluid in the direction of the *increment* of the density of the substance, and, as to amount, independent of the direction of the stream relative to the direction of the increment of density. On these principles it may be shewn, by simple mathematical reasoning, that this accelerative force varies conjointly as the square of the velocity of the fluid, and as the increment of the atom-density in a given small interval.

89. If then a steady stream be supposed to permeate a cylindrical conductor having rounded ends and a gradual increment of interior density from one end to the other, it follows from the foregoing argument that the accelerative force due to the presence of the atoms will generate a *secondary* stream, which will *issue from the denser* parts and *enter into the rarer* parts. This stream must also satisfy the condition of moving in *circulating* courses, because the inertia of the surrounding mass of fluid, supposed to be of unlimited extent, would put a stop to continuous motion in a given direction.

90. But as regards the phenomena of electrical attractions and repulsions it is chiefly necessary to consider the case of *unsteady,* or *vibratory,* motion in the interior of a substance of varying density. The solution of the problem of the incidence of the vibratory motion of a fluid on a small sphere shews that, as in the case of steady motion, the quantity of fluid which is passing at any instant a plane perpendicular to the mean direction of the motion is not sensibly altered by the reaction of the sphere, nor by that of a congeries of spheres occupying by their dimensions an extremely small portion of the space containing them. Hence the variation of the atomic density will give rise to an impressed accelerative force in the direction of the increment of density just as in the case of steady motion. On obtaining, by means of the general hydrodynamical equation which gives the pressure in any case of variable motion, the expression for that accelerative force, it is found to contain a term of the same form as that which applies to steady motion, with an additional periodic term of the first order. Omitting this term as having no influence in the present enquiry, the accelerative action varies, as before, conjointly as the square of the velocity of the fluid, and the increment of atomic density in a given small interval. But it is to be noticed that *this* velocity is expressed by the sine or cosine of a circular arc, and its square will, therefore, be partly constant and partly periodic. The periodic part will have just as much positive as negative effect, and on that account will not modify the result; but the constant part will be operative in just the same manner

as the square of the velocity in the case of steady motion. Consequently, if the motion of the æther in the cylindrical conductor consists of a *regular* series of harmonic vibrations, the variation of its density from end to end will generate circulating streams of *steady* motion, issuing from the parts about the denser end, and entering into those about the rarer end[1].

91. The remainder of the argument depends on the following two hydrodynamical propositions which admit of easy proof; (1) that different small steady motions of a compressible fluid can coexist; (2) that the composite motion is steady motion, the relation between which and the resultant density is expressible by a formula of the same kind as that which expresses the relation between the velocity and density of each component. These inferences from the mathematical theory of the motion of an elastic fluid being taken for granted, the explanation of electrical attractions and repulsions will proceed as follows. It is, first, to be remarked that the variation of condensation from point to point of the surface of an atom immersed in a steady stream depends in no perceptible degree on the reaction of the sphere, because in the case of the incidence of *waves* the condensation produced by the reaction of the sphere is so much the less as the breadth of the waves is greater, and a stream may be regarded as a portion of a wave of infinite breadth. Consequently the condensation at the surface of the atom will be the same at the same points as if the atom were absent, and the moving force of the pressure of the fluid upon it will be the same as that on a sphere of the fluid of the same size and in the same position, and will act always in the direction of the decrement of density.

92. This being understood, let us now trace all the consequences of causing a neutral conductor to approach a fixed conductor electrified either positively or negatively. According to the foregoing theory the former will be electrified inductively by

[1] The reasoning which I have here attempted to express without having recourse to literal symbols is conducted mathematically in an Article on "The Hydrodynamical Theory of Magnetism," contained in the *Philosophical Magazine* for June, 1872.

the other, which will then be acted upon in turn by the induced electricity, and so on, till by mutual inductions the electricities of both are brought, as to amount and superficial distribution, to certain states depending on the distance between the bodies. At varying distances the parts turned towards each other will be *oppositely* electrified, so that the secondary stream of one will be entering while that of the other is issuing, and the two streams will accordingly move in the *same* direction. It may, therefore, be assumed that by the composition of the streams there will be a *maximum* of velocity, and by consequence a *minimum* of density, at some position between the bodies. Hence the atoms of each body will be urged, upon the whole, towards that position, because the condensation of the fluid will be unsymmetrical relatively to the middle sections of the conductors. Consequently as the electrified conductor was supposed to be fixed, the moveable one will be caused to move as if *attracted* by it.

93. By analogous reasoning, if any two electrified bodies be brought into proximity, and the parts turned towards each other have the *same* electricity, the respective secondary streams will either issue or enter in *opposite* directions, and by composition of the velocities there will be a *minimum* of velocity and a *maximum* of density at some position between the bodies. The accelerative action of the æther on the atoms will, accordingly, tend upon the whole to drive them farther apart, and thus there will be an apparent mutual *repulsion* between the bodies, both of them being supposed to be moveable.

Thus *electric attraction* and *electric repulsion* are both accounted for by reasoning founded exclusively on the adopted hypotheses.

94. If two electrified bodies, having, for instance, the spherical form, be drawn by mutual attraction so as to come into actual contact, immediately their electricities will be transformed by a new distribution over the surfaces of both, and become the same as if the two spheres constituted one body. Consequently, according to the conditions of electric equilibrium explained in Arts. 73 and 74, the electricity at the point of contact

will be zero, and on the circumjacent surfaces confronting each other the electricities will gradually increase with the distance from this point, and will be of the *same* kind, the forces acting on any atom of either superficial stratum being in equilibrium on the supposition that the two spheres are *attached* at the point of contact. Hence, if they be unattached they will be separated by the mutual repulsion due to the like electricities. This result accords with experiment.

95. To complete this theory of electrical attractions and repulsions it will be proper to explain farther why the ætherial vibrations which I have called *electricity-radiants,* although theoretically capable of causing a substance composed of atoms to move bodily, produces no such effect comparable with that resulting from the variations of condensation due to the steady motions above considered. (See Arts. 86 and 87.) It has already been observed that the condensed and rarefied parts of a wave have opposite and nearly equal dynamical effects, and that the differential force to which motion of translation is ascribable varies as the square of the maximum velocity of the fluid, and must in any case be extremely small unless the waves be of very large breadth and intensity. It is to be considered that even the translatory forces of gravity-waves between bodies of *moderate* size can only be detected by the most delicate experiments. Besides, as has already been indicated, the translatory action of the electricity-waves may be chiefly expended in maintaining against opposing forces interior gradation of density.

96. But with respect to the secondary steady motions generated by means of the interior gradation of density of electrified bodies, it is particularly to be noticed that the impressed accelerative forces due to the occupation of space by the atoms act upon a *given* particle of the fluid in the same direction during the whole of its course through the electrified body, and thus the maximum velocity generated may far exceed that of the electricity-radiants. Accordingly the attractions and repulsions, which the theory attributes to the variations of density from point to point of space, due to steady motions so produced, might be much

more prominent than those ascribable to the electricity-radiants, and might render the latter altogether indistinguishable in the combined result of their simultaneous actions.

The foregoing discussion has, I think, sufficiently shewn what, according to the adopted hypotheses, is the intrinsic nature of electricity, and it is, therefore, unnecessary for my purpose to say more on this head[1].

97. MAGNETISM. The reason that the theory of magnetism may be suitably placed for discussion next after the foregoing theory of electricity will be apparent from the explanation I am about to give of magnetical attractions and repulsions. I assume, as the fundamental hypothesis of magnetism, that in the interior of a magnetized bar of steel there is a regular and very small gradation of density from end to end. On this assumption the generation of secondary streams may take place in the manner already described relatively to an electrified body, and the mutual attractions and repulsions of magnetized bodies may, like those of electrified bodies, be caused by variations of the density and pressure of the æther from point to point of space due to the coexistence of their steady secondary streams.

98. But after noticing these analogies between the causes, as regarded from the point of view of this hydrodynamical theory, of the motions of electrified and magnetized bodies, essential points of difference have to be taken into account. The assumed gradation of interior density cannot be ascribed, as in the case of electricity, to the mechanical operation of friction, because it is found to exist *naturally* in certain bodies, and in an eminent degree in some kinds of iron. It is out of the province of this theory to enquire what may be the particular constitution of iron which renders it more susceptible of magnetism than other substances, but it is essential to ascertain by what action of the ætherial forces the assumed gradation of density is maintained.

[1] The theory of Electric Force given in my work on *The Principles of Mathematics and Physics* differs in several particulars from that here produced, which, although it rests fundamentally on the same principles, I consider to be more simple, and more satisfactory in its details, than the other.

Now this question, as I conceive, is answered by saying, that, *in general,* the resultant of the atomic repulsions acting on any atom in equilibrium along any straight line drawn through its centre is zero, and that the case is the same with respect to molecular attractions of the first order; but that in certain substances, so constituted as to admit of a small permanent gradation of interior density, the equilibrium of an atom is maintained by an equality between the resultant of the atomic repulsion in the direction of the *decrement* of density and that of the first-order molecular attraction in the opposite direction.

99. This view being admitted, it follows that the generation of the secondary steady streams is ascribable exclusively to the *first-order* molecular attraction-waves, and that in magnetized bodies those of the second-order have no perceptible influence. This conclusion is confirmed by the fact that a magnet exhibits no electricity, for the existence of which, according to the preceding theory of electricity, second-order molecular attractions causing the superficial atoms to take new positions of equilibrium are necessary. Consequently, in this respect electricity and magnetism are essentially distinct. The magnetic secondary streams depend altogether on the capability of the atoms of a magnet to take positions of equilibrium consistent with the condition of a regular gradation of interior density without the employment of any exciting cause like friction. The proper process of magnetization will be considered under the head of the induction of magnetism.

100. This theory furnishes the means of giving at once explanations, as follows, of several well known facts of magnetism.

(1) Since, from the mode of generation of magnetic streams, those of a bar magnet are made to pass out at and near the end towards which the density increases, and to enter at and near the other end, the theory accounts for the observed difference between the phenomena at the two ends, which is expressed by saying that the magnet has *poles,* one *positive* and the other *negative.* I shall assume that the one which is called the *positive* pole is that from which the streams *issue.*

(2) According to the hydrodynamical principle already indicated, that the inertia of an unlimited mass of fluid prevents a continuous flow in one direction, the streams must *circulate*, and, therefore, satisfy the condition of entering at the end of the magnet opposite to that from which they issue. In short, the theoretical courses might very well be supposed to agree with the directions shewn by the small magnets in the diagram in page 17 of the Astronomer Royal's Treatise on Magnetism, which directions were determined by actual experiment. The same principles account for the magnetism of a steel bar being equal and opposite on the opposite sides of a middle neutral position, the amount of accelerative force opposed on one side to the *reluctance* of the fluid to motion on account of its inertia, being equal to that opposed on the other side to its *resistance* to motion from the same cause.

(3) If a magnetized bar be divided transversely into two or more parts each part will become a magnet, the gradation of density being supposed to remain in it after the separation. It is found, in fact, that the divided parts of a magnet are magnets, and accordingly the supposition that the gradation of density continues in the separate parts is justified.

(4) The experimental law that "like poles repel and unlike poles attract" is accounted for, as already intimated, by the same hydrodynamical laws of pressure in steady motions as those which were applied in the explanation of electrical attractions and repulsions.

(5) In general the direction of the gradation of density will depend on the form of the magnetized body. If it has the form of a horse-shoe the trajectories of the surfaces of equal density will be nearly parallel to the axis, and the ends brought into proximity will be opposite poles. In the case of a magnetized steel ring, the gradations of density would clearly be symmetrical with respect to a plane passing through the centre of the ring and the positions of maximum and minimum density; so that the two halves would generate equal and opposite magnetic streams, which, flowing like the stream in the curved part of a horse-shoe magnet,

would have no outlet, and consequently would neutralize each other. It is found, in fact, that a ring under these circumstances gives no indication of magnetism, whilst, if it be broken transversely into pieces, each piece, as might be anticipated from the theory, becomes a magnet[1].

101. Other explanations of magnetic phenomena on the same principles might be adduced. I shall limit myself to adding one which will test in a special manner the truth of the theory; namely, an explanation of the experimental results on which Gauss has founded his demonstration that magnetic force acts according to the law of the inverse square[2]. These experiments, and the inferences from them which it is proposed to account for by our theory, may be briefly stated as follows. A large magnet having been fixed with its axis transverse to the plane of the magnetic meridian, a smaller one, about a foot long, moveable about a vertical axis, and having its centre in the horizontal plane passing through the centre of the other, was placed so that its axis, when parallel to the magnetic meridian, pointed exactly to the middle of the large magnet. The deviation under these circumstances of the axis of the small magnet from the meridian, caused by the action of the large one, was then measured. The small magnet was next placed in a position such that its centre was in the same horizontal plane as before, and at the same distance from that of the large magnet, and the axis of this magnet pointed exactly to its centre. Under these new circumstances the deviation of the axis of the small magnet from the magnetic meridian due to the action of the other was measured as before. These operations having been gone through for fourteen distances between the centres, varying from four to thirteen feet, the calculated results shewed that in the second arrangement the angular deviation for any distance was very approximately *twice*

[1] The facts relating to the magnetized ring are stated in Art. 599 of Brooke's *Elements of Natural Philosophy*.

[2] A translation of Gauss's Memoir on the "Absolute measure of the Intensity of Terrestial Magnetism" (Göttingen, 1833), is published in the *Annales de Chimie et de Physique*, Tom. 57, 1834.

as great as that in the first for the same distance, and that in both series of results the deviations at different distances varied inversely as the *cubes* of the distances.

102. In order to account for these facts it is, first, necessary to explain in what manner the magnetic streams of the fixed magnet produce the movement of the other about its vertical axis. It will be assumed that the streams of each magnet circulate in the manner described in Art. 100 (2), so that where they cross externally a plane transverse to the axis through the centre of the magnet, the motion is parallel to the stream along the axis from the negative to the positive pole, but in the *opposite* direction. Also it is to be understood that the *positive* pole of a horizontal needle in the magnetic meridian, the one from which the streams are supposed to *issue*, points *southward.*

103. For the first arrangement, conceive the *positive* pole of the fixed magnet, having its axis transverse to the magnetic meridian, to point *westward,* and the *negative,* or *north,* end of the axis of the small one to point exactly to the centre of the first. Then, since the streams issue from *positive* poles, those issuing from the fixed magnet will at first have a *westward* course, and after being turned into the opposite direction, will be incident on the *west* side of the rotatory magnet. Hence they will conspire on that side with the horizontally resolved streams entering at its *north* end, and be opposed to the horizontally resolved streams issuing at its *south* end; and the contrary will be the case on the opposite side. Hence there will be an excess of pressure (the pressure being less as the velocity is greater) on the *east* side of the *north* end, and on the *west* side of the *south* end. The atoms at this end will therefore be driven eastward, and the rotation will be contrary to that of the hands of a watch.

104. For the second arrangement, the axis of the fixed magnet being, as before, transverse to the magnetic meridian, let its *negative* end now point westward, and exactly towards the centre of the rotatory magnet. In this case the *entering* streams of the large magnet will be incident on the *west* side of the other; and by just the same reasoning as in the former case the south

end of the latter will be driven eastward, or the movement will again be contrary to that of the hands of a watch.

105. It is readily seen that by reversing the fixed magnet *four* different sets of relative positions of the poles of the magnets might be obtained of the first kind, and as many of the second. It is not necessary for my purpose to consider more than one of each kind. Those assumed above were selected because of their coincidence with the arrangements represented by Figures 9 and 10 in pages 25 and 28 of the Astronomer Royal's Treatise. As he states that the directions of the angular displacements were in both cases "against watch-hands," it follows that the results of the present theory are so far confirmed by experiment.

106. It is a more difficult problem to determine on hydrodynamical principles the velocity and direction of the streams of the fixed magnet at all distances from its centre; but the following reasoning, although it is not exact, appears to be sufficiently complete to test by its results the truth of the theory. It has been argued (Arts. 97 and 99) that the steady streams of a magnet owe their origin to a regular gradation of density from one end to the other, and to the maintenance of this condition by molecular attraction of the first order, acting in the direction of the *increment* of density, and counteracted by atomic repulsion. It was shewn, on the principle of constancy of mass, that under these circumstances the gradation of density would act as an extraneous force always accelerating in the direction of the increment of density, and that the resulting motion of the fluid would be partly steady and partly vibratory, the steady part accounting for magnetic streams. These streams, it was argued, must flow in circulating courses, because the inertia of a mass of fluid of unlimited extent would prevent any continuous transfer of fluid across any plane having a fixed position in space. But the particular velocities and courses by which this condition might be fulfilled were not determined. This I shall endeavour to do now by employing the following reasoning.

107. On the adopted hypothesis that the atoms are separated from each other by comparatively large intervening spaces, it is

evident that they will severally, by the reactions at their surfaces, give rise to independent secondary motions, and that the results of the composition of these coexistent motions ought to be identical with the motions attributed above to the *mean* accelerative effect of the variation of atom-density, and to the *mean* reaction due to the inertia of the fluid. Now Poisson's solution of the problem of the simultaneous movements of a ball-pendulum and the surrounding fluid will enable us to indicate precisely the secondary motions due to each atom[1]. Let us suppose, at first, that the atom moves in the æther at rest. Then from that solution it follows (1) that the *normal* velocity of the fluid at any point of the surface of the sphere is equal to the product of the velocity of the sphere and the cosine of the angle which the radius to the point makes with the *positive* direction of the motion, and consequently, at the point for which this angle vanishes, is equal to the sphere's velocity; (2) that the *tangential* velocity at the same point is equal to the product of the sine of the same angle and *half* the velocity of the sphere, and at the points for which the angle is 90^0 is *opposite* in direction to the motion of the sphere; (3) at any distance from the centre the velocity along the prolongation of the radius, and that parallel to the tangential velocity, both vary inversely as the *cube* of the distance.

108. But in the case of our problem the sphere does not move in the fluid at rest, but waves of the fluid are incident on the sphere at rest, namely, the first-order attraction-waves propagated in the magnet in the direction from the denser to the rarer end. The analogous solution for this case gives, as might have been anticipated, the result that the disturbance of the fluid by the fixed atom *relatively* to the fluid in motion, is the same, *quam proxime,* as if the atom had in the fluid at rest a motion equal and opposite to the actual motion of the fluid. Now as the incident waves are propagated from the denser to the rarer parts of the magnet, the motion in the condensed part of a wave is in that direction, and the *relative* motion of the atom in the contrary direction. Hence the atom urges the condensed part of the wave

[1] See *Principles of Mathematics and Physics*, pp. 260—264.

towards the denser parts of the magnet, and the rarefied part towards the rarer, and the quantities immediately urged in a given small interval of time at any position will vary as the product of the velocity of the sphere and density of the fluid at that position. Hence, since by the laws of ætherial waves the velocities are *equal* and opposite at corresponding points of condensation and rarefaction, the excess of the flow of a condensed portion of a wave towards the denser end of the magnet, as caused by the reaction of the sphere, above the flow of the corresponding rarefied portion in the opposite direction, will vary as the product of the common velocity and the difference of the densities, that is, as the *square* of the velocity, because the difference of the densities varies as the velocity. Consequently the total differential flow in the course of the transit of a complete incident wave varies as the sum of the squares of the velocities in the half (or whole) of the wave. But this sum is partly constant and partly periodic, the constant part being proportional to the square of the maximum velocity. Hence, as the periodic part has as much negative as positive effect, the mean flow towards the denser end of the magnet will be proportional to the square of the maximum velocity of the incident waves. (See an analogous argument in Art. 56.)

109. Now if the magnetized bar had the same density throughout, this effect of the reaction of the atoms would be the same at all transverse sections, and there would be a mean uniform flow of the fluid, but no acceleration of a given particle. Such impressed motion would be quickly stopped by the inertia of the external fluid. But on the hypothesis of a regular gradation of density the resulting mean velocity would be greater, the greater the density, on account of its being due to the reactions of a greater number of atoms. In that case the motion of a *given* particle would be accelerated towards the denser end, and steady circulating motion would be the mean result of the composition of the individual motions. According to this argument and that in the preceding paragraph, the acceleration varies conjointly as the square of the maximum velocity in the incident waves, and the variation of the density of the bar in a given small space parallel

to its axis; for if either of these factors were zero the acceleration would vanish. We have thus arrived at precisely the same results as those obtained (Art. 88) when only the *mean* effect of the occupation of space by the atoms was considered; for, in fact, the two investigations depend alike on the principles of constancy of mass and circulating movement. But the second investigation is capable of giving, in addition, information respecting the velocity of the fluid at any point and the courses of the streams. It will suffice for my purpose to deduce only the following consequences.

110. From what has now been shewn it may be inferred that if we take within the magnet a rectangular prismatic space which, although containing a very large number of atoms, may be regarded as elementary, the sum of the motions due to the reactions of all these atoms will be partly a continuous flow, and partly vibratory motion. Also, since the two kinds of motion are produced by impulses relatively communicated to the fluid by small spheres separated by comparatively large spaces, they will have common courses, and the velocities will vary with distance from the centres of the spheres according to the same law. This, in fact, may be considered to be proved by Poisson's solution above referred to, which is applicable whether the impulses be continuously in the same direction, or vibratory, or be compounded of impulses of both kinds. Further, it is evident, on the principle of the coexistence of small motions, that the composite secondary motions emanating from the above-mentioned prismatic space, both the flowing part and the vibratory part, will, *quam proxime*, take the same courses as the secondary motions from a single atom placed at the *centre* of the elementary space, and will vary with the distance from the centre according to the same law. As far as regards the dynamical action of the æther, which is the object of this investigation, the steadily flowing composite streams will alone be operative, and the vibratory motions, as producing equal effects in opposite directions, may be left out of consideration.

111. This being understood, we may go on to argue that the result of compounding the composite steady motions from all the

elementary spaces comprised within the extent of the magnet will be steady motions *approximately* coinciding, as to courses and law of variation of the velocity, with those proceeding from the single elementary space the centre of which coincides with the centre of the magnet. This approximation to the case of a single element will clearly be closer the smaller the dimensions of the disturbed magnet, and the greater the distance between the centres of the two magnets. This limitation is indicated by the experiments themselves, which shew that the laws deduced, as already stated, from experiments made at considerable distances, are not satisfied by those made at small distances.

112. From the whole foregoing argument it may now be concluded that the theory is in agreement with experiment as regards (1) the direction of the angular displacement of the disturbed magnet by the magnetic influence of the disturbing one; (2) the variation of the amount of displacement according to the law of the *inverse cube* of the distance between the centres of the magnets; (3) the ratio of the amount when the disturbing magnet points to the middle of the disturbed magnet to that for the same distance when the latter points to the middle of the former, namely, the ratio of 2 to 1.

113. Gauss has, besides, inferred from his experiments that magnetic action varies according to the law of the inverse square, by arguing on the hypotheses that there are *two* magnetic fluids, and that the elements of one fluid attract those of the other, while two elements of the same fluid are mutually repulsive. These hypotheses are virtually adopted by the Astronomer Royal where (in pp. 10 and 11 of his Treatise) he assumes that in magnetic action there is a "Duality of Powers," and that like magnetisms mutually repel and unlike mutually attract. I am precluded by the principles of the Philosophy I am upholding from giving any consideration to these hypotheses, simply for the reason that they are not comprehensible by any kind of antecedent experience, and require explanation as much as the phenomena they are intended to explain. But I grant that it is incumbent on me to account for the laws immediately deducible from Gauss's

experiments by employing reasoning founded on the hypotheses I have adopted, which satisfy the condition of being intelligible from sensation and experience; and this I maintain that I have done by the argument now concluded. This point I insist upon the more, because it well illustrates in what respect especially the Newtonian Philosophy I advocate differs from systems which are more favoured by physicists of the present day, being essentially distinguished from them by the character of its hypotheses. The important numerical quantities which have been derived from the Gaussian experiments, and the determination of the absolute Intensity of Terrestrial Magnetism, equally belong to my view of magnetic force, and have no necessary relation to the deduction of the law of the inverse square from the hypotheses adopted by Gauss and Airy.

114. *Induction of Magnetism.* For the theoretical explanation of the class of phenomena under this head, it is chiefly important to remark that as the streams from any magnet flow freely through other substances, and as the pressure in these streams varies from point to point of space, they tend always to disturb the equilibrium of the atoms of the substances which they permeate. In some bodies, as especially soft iron, the disturbed atoms immediately assume new positions of equilibrium, which they retain so long as the disturbance lasts. It is a necessary condition of this equilibrium that the disturbing force, in conjunction with the intrinsic forces of the body, should maintain a regular gradation of atom-density, such that the trajectories to the surfaces of equal density have forms depending on the shape of the body. By this means the body is converted into a temporary magnet, having poles and its own magnetic streams. Consequently since, as will presently be shewn, magnetic streams have under these circumstances a *directive* as well as an inducing power, the streams of the inducing magnet will cause the positive or negative pole of the temporary magnet to be directed towards its own contrary pole. In either case *attraction* ensues. This theory accounts for the attraction of iron filings equally at *both* ends of a bar magnet, and the formation of the *tufts* by the

mutual attractions of the filings converted into temporary magnets.

115. The property of being susceptible of magnetism, which according to the present theory depends wholly on the atoms of a body being capable of taking new positions of equilibrium under the condition of a regular gradation of atom-density, is found to be possessed in different degrees by many substances of various kinds, but to exist in none so conspicuously as in soft iron. There are other bodies which shew no trace of magnetism, and in some even an opposite property, which might be called a *negative* magnetism, has been detected. Speaking theoretically, it may be said of this property, that under circumstances in which a gradual increase of atom-density would be produced in a certain direction in soft iron, a gradual decrease is produced in the same direction in a piece of *bismuth*, the effect lasting only during the disturbance. Substances of this class are called *diamagnetic*.

116. The peculiarity of *steel* is, that while its atoms are not susceptible, like those of soft iron, of being readily displaced by a magnetic stream, they can be made to take new positions of equilibrium by *repeated* disturbances always in the same direction, and when a new state of equilibrium has been thus established, it remains permanently, or with more or less persistence, after the discontinuance of the disturbances. This is the principle of the process of *magnetization*. When performed on a steel bar, a magnet is usually drawn along the surface of the bar from end to end, and repeatedly towards the same end. The magnet is moved with one end in actual contact with the bar, because the effect is greater the nearer the pole of the magnet is to the bar, and the same end is used throughout the process, because the two ends produce opposite effects. Our theory gives, as the reasons for these facts, that as the streams of a magnet enter at one end and issue at the other, they produce opposite magnetizing effects at the two ends, and that the effect is intensified in the neighbourhood of a pole, because the courses of the streams are there more curved, and the variations of pressure from point to point larger, than at more remote positions. Magnetizing to *saturation* means that the

displacements of the atoms have reached a limit imposed by the constitution and intrinsic forces of the magnetized body.

117. According to the theory also, since streams that magnetize act only by variations of the pressure of the æther in steady motion, they are not restricted to those of a magnet: streams from any other source, if they satisfy the same conditions, would produce the same effect. It is found, in fact, that iron can be magnetized temporarily, if not permanently, by *galvanic* streams. To this point there will be occasion to recur.

118. The *directive* influence of a magnetic current on a magnet suspended so as to be freely moveable in any direction about its central point, is explained by the theory as follows. Conceive the current to be, at first, exactly in the direction of the axis of the magnet, so as to flow in the *mean* direction of the portions of the magnet's streams which enter at its negative end and issue at its positive end. Then as these streams are symmetrical with respect to the axis of the magnet, their coexistence with the given current will have no disturbing effect on the position of the magnet. Now let the axis receive a small angular displacement in any plane passing through the magnet's centre, and let its streams be resolved in planes parallel to that plane, and these resolved parts be again resolved in planes transverse to the axis. Then by taking account of the courses of the magnet's streams it will be seen, that the given current resolved in the same transverse planes will be opposed to the last resolved magnetic streams at the parts of the magnet which are farthest from the original position of the axis, and coalesce with them at the opposite parts. Hence by an argument previously employed (Art. 103), the magnet will be urged on both halves towards its original position. Thus the effect of the current is to check any angular displacement of the magnet, and accordingly to determine the direction of its axis.

119. The foregoing theory also shews why, in the experiment of the attraction of iron filings by a magnet, the axes of the temporarily magnetized filings all lie along the lines of motion of the magnet's streams, and have their positive poles all turned in

the direction of the flowing of the streams, both of those entering the magnet, and of those issuing from it.

120. *Terrestrial Magnetism.* In attempting to give theoretical explanations of the phenomena of this extensive and complicated department of physics, I am forbidden, by the philosophical principles I have adopted, to make any other hypotheses respecting atoms and the æther than those already employed. Accordingly it will be supposed that the earth consists wholly of discrete atoms having only the properties defined in Arts. 26 and 27; that the occupation of space by the atoms is very small compared to the intervening spaces even at the earth's centre where the atom-density is greatest; and that the æther at rest has the same density and elasticity within the earth as in the space outside. Also atomic and molecular forces of different orders, the same as those to which the equilibrium of the atoms of other bodies was attributed, will be supposed to be in operation in the earth; and besides, with respect to so large a mass, the effect of the force of gravity has to be taken into account.

121. Let us, first, consider the motions which the atoms, by partaking of the earth's *orbital* motion, impress on the æther. Those due to the motion of any single atom may be at once inferred from the solution of Poisson's problem of the ball-pendulum previously referred to, and on the hypothesis that the proportion of space occupied by the atoms is extremely small, the total motion is, *quam proxime*, the sum of all the motions produced by individual atoms. Also since according to that solution just as much fluid flows backwards as the atom displaces by its forward motion, it follows that during the time the earth is moving over a portion of its orbit equal to its diameter, a quantity of fluid crosses the transverse plane which is a common tangent to its surface in the first and last positions, equal to the quantity which would just fill the space occupied by the atoms. Hence if we supposed this quantity to flow in that interval in a uniform concentrated stream through a circular aperture of radius equal to that of the earth, it might readily be shewn that the ratio of its velocity to the earth's velocity is two-thirds of the ratio of the

space occupied by atoms to the earth's volume. This ratio, according to hypothesis, is extremely small, and, therefore, it would not be contradictory to the theory to find that but little magnetic effect is attributable to æthereal currents at the earth's surface generated by the orbital motion.

122. The motions resulting from the composition of all those severally impressed by the atoms will be circulating streams because the components are such, and will evidently be symmetrical with respect to an axis passing through the earth's centre in the direction of the motion, fulfilling at the same time the condition that just as much fluid enters at the following hemispherical surface as issues at the preceding one. The total motion will consequently be a *circulating* motion having always the same relation to the position of the earth's centre, and a plane through the centre transverse to the direction of the motion will be crossed by the *refluent* streams at right angles. The disturbances of a magnet's declination producible by these currents would have two maxima and two minima in the course of a revolution of the earth about its axis; and together with this *diurnal* variation, there would be some amount of *annual* variation on account of the obliquity of the ecliptic.

Like considerations are applicable to disturbances of the æther which may be caused by a uniform rectilinear motion of the earth in space in common with the solar system. Under these circumstances a diurnal, but not an annual, variation of magnetic declination might be produced.

123. According to the proposed theory of attractive and repulsive forces (Arts. 60 and 62), the equilibrium of every atom of the earth is separately maintained by the counteraction of attraction-waves tending to draw it towards the earth's centre by repulsion-waves urging it towards the surface. As this equilibration of the atoms is accompanied by a regular increment of atom-density towards the centre, there is exactly the same reason for the production of steady ætherial currents tending towards the denser parts, as in the case of the gradation of atom-density of a magnet (see Art. 98). Now if the earth were a sphere of homogeneous

material, these currents, tending equally in all directions towards the centre, would there be stopped, and being reflected back would tend to flow out equally in all directions at the surface. But such motion could not actually take place, being prevented by the inertia of the surrounding mass of fluid. Consequently, only so far as the condition of the earth differs from that of a homogeneous sphere can steady streams be generated by the interior gradation of atom-density. The phenomena of terrestrial magnetism do not, however, forbid the supposition that the magnetic streams may to some amount be due to gradation of density combined with heterogeneousness of the materials, as land and water, which compose the earth's superficial parts. But the influence of this cause on the direction and intensity of the streams would be mixed up with one, probably much more powerful, arising from the earth's rotation, to which attention will be subsequently directed.

124. By a like argument, the density of the earth's *atmosphere*, so far as it is a function of the distance from the centre, would not, by its variation with the distance, give rise to magnetic streams. Also if the density were symmetrical with respect to the plane of the equator, and a function of the latitude, its gradations would tend to produce only mutually neutralizing streams. The result would be different in the case of gradations of density produced by the varying action of the Sun's heat, which, as I shall next endeavour to shew, might be productive of a sensible amount of magnetism.

125. The Sun being supposed, for the sake of simplicity, to be in the plane of the Equator, its heat will cause, at the earth's surface, an expansion of the atmosphere, the degree of which will be less the greater the distance from a certain position of maximum heat and expansion, and may be considered to be approximately a function of the superficial distance from that position. [As far as regards the theory of magnetic *declination*, which is the object of this investigation, the effect of vertical expansion may be left out of account.] The greatest heat, as is known, will not be at the place to which the Sun is vertical, but one some 15^0 to the east of

it. The expansion is maintained entirely by atomic-repulsions, the condition of equilibrated action being that the intensity of the repulsion waves from any single atom is inversely proportional to the atom-density where it is situated. But attraction-waves both of the earth and the atmosphere itself traverse the parts in which a gradation of density is produced by unequal expansion due to the Sun's heat. Hence, according to the theory, as already explained, the conditions are proper for generating ætherial streams, and the generating accelerative forces all tend from parts rarefied by heat towards those which are, *cæteris paribus,* denser as being colder.

126. But clearly these forces would be prevented by the inertia of the fluid from causing it to flow continuously from the same portion of space. Consequently if the forces acted *equally* in all directions from the position of greatest heat the flowing would be stopped. Since, however, that condition is not actually fulfilled by the effect of the Sun's heat on the atmosphere, there will be a differential action whereby a current will be produced subject to the condition that as much fluid issues from, as enters into, a given space; that is, it will be *steady circulating motion.*

127. This being understood, we may proceed to consider what might be the character of the lines of motion of the current so generated. Supposing the expansion of the air by solar heat to be *approximately* a function of the distance from the hottest point of the equator, the lines of motion would be nearly alike on the opposite sides of a meridian through that point, having their convexities all turned towards that meridian, and would be cut symmetrically by the equator and cross it at right angles. Consequently there would be a diverging stream of the æther from the hottest parts of the atmosphere either northward or southward, and a converging stream into the same parts southward or northward, so that the lines of motion would be closest together in the hot parts and most asunder in the opposite cold parts. Thus the magnetic effect of the current will be much more considerable in the day-time than in the night-time. This accounts

for the small amount of the solar diurnal variation of magnetic declination in the hours of the night.

128. It is not possible by theory alone to determine whether the streams flow northward or southward; but this point may be ascertained by combining as follows the foregoing theory with observation. Taking a morning hour, as 8 o'clock, and assuming the flow to be northward, since the course of the current will according to the theory be then towards north-west in the northern hemisphere, by being compounded with the southward terrestrial current it would produce a deviation of the *north* end of the needle eastward. So a combination of the atmospheric current at the same hour at a corresponding position in the southern hemisphere (where it would flow towards north-east) with the southward terrestrial current, would cause the *south* end of the needle to deviate towards the east. These two results agree with observation, and accordingly, the supposition that the ætherial current flows *northward* is confirmed. Possibly the reason may be, that the distribution of land and ocean makes the difference between the equatorial and arctic temperatures to be greater than that between the equatorial and antarctic.

129. According to the foregoing theory of the magnetic effect of the Sun's heat, *easterly* deviations of the *north* end of the needle will generally prevail in the *northern* hemisphere in the hours before the epoch of maximum heat, and westerly declinations in the subsequent hours. Observation confirms this law in a general manner, while it indicates that causes exist which disturb its regularity. Also the deviations, according to the theory, will again be easterly before the night epoch of minimum temperature, and westerly after that epoch; but the nocturnal variations will be very much smaller than those in the day-time. On this account it is possible that the causes which modify the day-variations may entirely change those of the night. Among these causes is to be reckoned the generation of refluent streams by the earth's orbital motion, as described in Art. 122. These streams, as may be easily seen, would produce a maximum westerly declination at midday and a maximum easterly declination at midnight, inasmuch as

they flow in the direction *contrary* to that of the earth's motion. The consequent movement of the needle would be westward from midnight to noon, and eastward from noon to midnight, passing through zero at 6 A.M. and 6 P.M. When, however, this movement is combined with those which have been supposed to be due to the currents generated by temperature, it is found that the observed movements of the needle are not truly represented. It may, therefore, be concluded that the needle is acted upon by currents due to some other cause or causes. The elucidation of this point must be deferred till we have taken account of the disturbance of the æther by *the motion of the earth about its axis*, which, in fact, appears to be the principal cause of terrestial magnetism. The origination of the streams due to the earth's rotation may be conceived to be such as follows.

130. As the resultants of the movements immediately impressed on the æther by the revolution of the atoms about the earth's axis are circular, there will be no such refluent streams due to the inertia of the general mass of the æther as those produced by the earth's motion in its orbit. In fact, so long as there is any *relative* circular motion between the æther and the atoms, the latter will continually impel the æther, till the relative motion is reduced to zero. It may, therefore, be inferred that in the ultimate, or steady, state of the motion, the *circular* motion of the æther within the earth is the same as that of the atoms by which it is immediately impressed. This circular motion will, by reason of the centrifugal force, operate upon the æther external to the earth, and the condition of steadiness requires that the external æther should partake of a certain amount of circular motion, in order that the whole tendency of the centrifugal force transversely to the axis may be counteracted by the inertia of the surrounding mass of fluid. In short, if we conceive to be impressed on the earth and the æther a motion equal and contrary to the earth's orbital motion, the result of combining this with the circular motion would be a composite *spiral* motion, having always the same relation to the earth at rest; and, according to hydrodynamical laws which admit of mathematical demonstration, this

spiral form of steady motion would be capable of travelling uniformly through space in company with the earth[1].

131. But it is evident that this rotatory motion will tend to draw the æther from the axis equally in all directions in any given transverse plane, and thus cause a tendency of the external æther to rush in at the polar parts to supply any vacancy which the centrifugal force might produce. If, however, the circumstances of the earth were in all respects symmetrical with respect to the equatorial plane, these tendencies would just counteract each other, and no currents, apart from the circular motions, would be generated; or rather, the currents which the influx at one pole would produce of itself would be just neutralized by those separately due to the influx at the other, and in consequence the influx at each would be stopped. But because the circumstances of the solid and fluid parts of the earth are not symmetrical with respect to the equatorial plane, there will be a differential action due to the centrifugal force, whereby streams will be produced which, as satisfying the necessary condition of *circulating*, will be permanent. The direction of the flow will be determined by the circumstances which violate the condition of symmetry; and it is, therefore, reasonable to suppose, since there is a preponderance of land in the northern hemisphere and the currents are always towards the positions of greatest atom-density, that they will flow out of the northern parts, and after circulating enter into the southern parts. This is the direction of the currents which was provisionally assumed in Art. 102.

132. Accordingly, the positions of greatest efflux and influx, although they cannot be far distant from the earth's poles, will be partly dependent on the distribution of land and water. It is, therefore, consistent with the theory to find that in the northern hemisphere there are *two* positions of maximum efflux, one, which at present is apparently the principal one, in a high latitude of the North American continent, and the other a little north of

[1] See a discussion of this Theorem in the *Principles of Mathematics and Physics*, pp. 563—566, and a more complete treatment of it, in an article on the Theory of Magnetism in the *Philosophical Magazine* for June 1872.

the Asiatic continent. There is not the same geographical reason for two positions of maximum influx in the southern hemisphere; and, in fact, observation has hitherto only established that if there be two they cannot be far apart. Hence the theory at once accounts for the circumstance that of all positions on the earth's surface the magnetic intensity is greatest in the neighbourhood of the south magnetic pole. For the total influx is necessarily equal to the total efflux, and, accordingly, the influx is intensified in that quarter either because there is but one position of maximum intensity, or, if there be two, because they are near each other.

133. We have next to take account of a possible action on the magnetic needle wholly different from those hitherto considered. Just as the earth by its rotation about its axis produces, in the manner described in Art. 130, circular gyrations of the æther, which in and near the plane of the equator may be supposed to extend to very great distances from the axis of rotation, the vast body of the *Sun* by its rotation in 25 days will give rise to analogous gyrations. Assuming this to be the case, and also that the space occupied by the gyrations is traversed by vibratory, or other motions of the æther, it would be legitimate to infer from the Undulatory Theory of Light, as based on hydrodynamical principles, that the space would be *lighted up*, and that thus the phenomenon of the *Zodiacal Light* might be accounted for. It is known from observation that the zodiacal light extends to distances from the Sun greater than the radius of the Earth's orbit. Consequently the magnetic needle would be acted upon by æthereal streams which, as being distinct from those originating at the earth, might be called *cosmical.* These streams, flowing in the direction of the earth's orbital motion, would be directly opposed to the refluent terrestrial streams described in Art. 122. It has already been stated that the magnetic effect of the latter, if of considerable amount, would not be in accordance with the observed diurnal movements of the needle. It must, therefore, be supposed either that the terrestrial streams have no such influence, or, if they have, that it is veiled by the opposite effect of the solar streams. On this supposition it will be found that

the day and night oscillations of the needle are sufficiently well accounted for, those depending on variations of atmospheric temperature in the day-time being partially modified by the solar gyrations, and those of the night being mainly determined by these gyrations. It seems probable from this investigation that neither the disturbance of the æther by the motion of the earth in its orbit *relatively* to the gyratory stream, nor that by the motion it has in common with the solar system, has any considerable magnetic effect.

134. It is found by observation that the diurnal variation of magnetic declination changes in amount with the change of the Sun's declination, or that there is an *annual* change of the diurnal variation. This fact is plainly consistent with the theory.

135. The question as to the cause of the *secular* variation of magnetic force hardly admits at present of theoretical treatment, because exact observations have not been continued long enough to allow of determining its law. I shall, therefore, only remark that on account of the asymmetry of the earth's magnetic streams relatively to its axis of rotation, the magnetic pole, *i.e.* the position at which the dipping needle is vertical, might be supposed to have an *oscillatory* motion about a certain mean position, and from what was previously argued respecting the influence of the two great continents of North America and Asia, that position would most probably be somewhere between them. This inference agrees with what is hitherto known of the secular changes.

136. So far as the foregoing theory indicates, there is no reason why the type of terrestrial magnetism should not be very nearly the same in successive years. But observation has shewn that it changes considerably from year to year. This is very conspicuously exhibited, as far as regards the magnitude and direction of the horizontal magnetic force, by the figures in plates XVI and XVII, in the *Philosophical Transactions* for 1863, pertaining to a communication (No. XIII) by the Astronomer Royal, entitled "On the Diurnal Inequalities of Terrestrial Magnetism, as deduced from observations made at Greenwich in the years 1841—1857." From subsequent observations it appears

that the forms of the curves for 1863 differ little from those for 1841, a fact which may be taken to be indicative of periodicity. The curves embrace variations of every kind (with the omission of some few that were exceptionally large), so that any of the variations which may be the same in consecutive years are represented in conjunction with others which, as the results shew, must change from year to year.

137. By an appropriate discussion of observations taken at Toronto and Hobarton, General Sir Edward Sabine separated the diurnal variations into two classes, one, the class of *regular* diurnal variations (the theory of which I have attempted to give above), and the other, a class of *disturbance* variations, obeying a law of daily maxima and minima wholly different from that of the former. He also detected the existence of a *periodical* variation of the diurnal disturbance-variations, and, finding the period to be about ten years, drew attention to the approximation of this period to that of the varying number of solar spots, as inferred from a long series of observations by M. Schwabe. Periodic variations of the magnetic elements have also been ascertained by other physicists, as especially Hansteen, who inferred from his own observations a period a little in excess of eleven years. We have, therefore, now to consider whether the present theory is capable of giving explanations of the disturbance-variations, and their periodicity.

138. In the first place it may be observed that the disturbances appear to be produced by æthereal streams which *cross* the regular magnetic streams. This view is suggested by the fact that magnetic disturbances are not unfrequently accompanied by displays of the *Aurora Borealis,* the phenomena of which are such as might be supposed to be due to transverse streams crossing the regular terrestrial streams at certain elevations above the earth's surface, and thus causing them to be *agitated* and *lighted up.* The existence of the transverse streams may be referable generally to the hydrodynamical law that in an unlimited mass of fluid steady streams necessarily circulate, so that those originally impressed within the earth or near its surface give rise to more remote return streams. If, however, the transverse streams be steady, no

luminosity would result from the crossing, because the composition of the two sets of steady streams would be steady motion. We have, therefore, to account for the existence of transverse streams which in greater or less degree are susceptible of *unsteadiness*.

139. If they have a terrestrial and local origin, such, for instance, as the varying temperature and density of the atmosphere due to the contiguity of differently heated seas and continents, it is easy to perceive that at a given position they will be of a variable character, and go through the variations in a solar day. Observations have been recorded which seem to justify the inference that the diurnal disturbance-variations are in part due to causes of this kind, and that some of this class, by reason of exceptional unsteadiness of the transverse streams, are accompanied by auroral phenomena extending over a limited area. So far as disturbance-variations are thus produced, they would have, in accordance with the results of observation, two epochs of maxima and minima daily. But with respect to such disturbances there is nothing to indicate that they would be subject to a law of periodicity, or that there would be a period, as stated above, of ten or eleven years. Accordingly the theory is required to assign some special reason for the existence and variability of streams to which disturbances of long period may be due. I shall now endeavour to shew how such effects may be produced by means of *planetary and solar influences*.

140. The Sun and the Sun's heat, as being symmetrical with respect to its equatorial plane, are incapable by themselves of generating magnetic streams. If, therefore, there are solar magnetic streams distinct from the gyrations previously spoken of, they must be due to external influence; and it does not appear that such influence can be exerted by any other bodies than the planets. Now there is reason from observation to conclude that the Sun is surrounded by a vast atmosphere, increasing in density towards the surface by reason of the gravitating force of the interior mass. Let us then assume that motions of the æther originating at a planet enter into this atmosphere either as vibratory motions to which the forces of light, heat, and gravity

are to be attributed, or as steady gyrations. According to the proposed theory of magnetism, each of these motions will, on account of the gradation of density of the solar atmosphere, give rise to magnetic streams, the effect of which will not be wholly neutralized by symmetrical action relative to the Sun's centre or equatorial plane. Also in consequence of the great extent of the gradation of density, the effective generated streams might be very much intensified, although the original motions should be extremely small; in the same way that the intensity of the magnetism of a magnetized bar is greater the greater its length, when the rate of change of atom-density in a given length is given.

141. The body of the Sun appears to be surrounded by a stratum of cloud-form matter, suspended in its atmosphere, and subject to continual changes by disruptions and accumulations, giving rise to the appearances that are called *maculæ* and *faculæ*. Since these changes are not referable to any action which is a function of the distance from the Sun's centre, it seems necessary to ascribe them to external influence; and, accordingly, they may reasonably be supposed to be produced by the action of magnetic streams having a planetary origin in the manner described above. The fact that the solar spots are for the most part limited to zones contiguous to the Sun's equator is in accordance with this view, the planetary influence being likely to be greatest in this region.

142. In order to account for terrestrial disturbance-variations by the action of these solar streams, it must be supposed that their intensity, as generated in the Sun's atmosphere is so great, that it is still of considerable magnitude at the distance of the earth. It is known, in fact, that large terrestrial magnetic disturbances have occurred nearly contemporaneously with the sudden appearance on the Sun's surface of luminous outbursts, which might be supposed to be produced by intense magnetic action. This theory would account at once for the periodicity both of the solar spots and of the magnetic variations, inasmuch as both kinds of phenomena would depend, as to amount, on the *configuration* of the planets, which will evidently be subject to a law of periodical recurrence.

In confirmation of this view it may be stated that the configuration of the three planets Jupiter, the Earth, and Venus, to which probably the planetary magnetic influence is chiefly to be ascribed, recurs, as far as regards its magnetic effect, in 10·4 years. (See *Principles of Mathematics and Physics*, p. 673.)

143. The disturbing action of these planetary-solar streams will depend on their direction relative to the regular terrestrial streams, being greatest when they cross the latter transversely. Consequently the disturbance-variations of declination will be continually changing at a given place, and might have two maxima and two minima in the course of 24 hours. Also as the directions of the terrestrial streams vary from point to point of the earth's surface at a given time, each place will have its own epochs of maxima and minima. In these respects the type of the solar disturbance-variations agrees with that of the terrestrial disturbance-variations considered in Art. 139, and the total disturbance-variations, as consisting of the sum of the two kinds, will partake of the same type; so that upon the whole there will generally be in 24 hours at each place two maxima and two minima of disturbance-variations at epochs proper to the place. This conclusion accords with the results of observation. It may be further said, just as in the case of disturbances of terrestrial origin (see Art. 139), that when the solar transverse streams due to planetary influence are exceptionally variable, exhibitions of Aurora may occur, which in this case are likely to be of more than ordinary intensity and extent. The *cosmical* class of streams appear, in fact, to account for those disturbances of the needle, called *magnetic storms,* which have been simultaneously felt at widely separated positions of the earth's surface, and which, according to the theory, are attributable to special solar magnetic conditions, whereby abnormal streams are generated of such intensity that their influence is prominent even at the distance of the earth.

144. It remains to account for another class of magnetic variations, which, having been found to complete their cycle in a lunation, have been attributed to the influence of the moon, and are called *lunar-diurnal* variations. Respecting these it may, in

the first place, be remarked that if planetary action on the Sun's atmosphere produce, in the manner supposed in Art. 140, solar magnetic streams, the action of the moon on the earth's atmosphere might be expected to produce in like manner terrestrial streams. In that Art. it was asserted that the streams might result from the entrance into the Sun's atmosphere of either vibratory or steady motions originating at the planet, but no attempt was made to determine exactly the mode of generation of the streams and their courses. This is, in fact, a very difficult problem; but perhaps the following considerations relative to the case of the moon may throw some light upon it.

145. From what has already been argued respecting the effect of the earth's motions (Arts. 122 and 133) it may be inferred that the disturbance of the æther by the moon's motion in space would give rise to no perceptible magnetism; and the motion about its axis is probably too slow to originate gyrations that would be felt at the earth. Besides, it may be shewn that the streams which these two motions might generate would produce magnetic variations not in accordance with the laws of the lunar-diurnal variations. With respect to the *vibratory* motions emanating from the moon, those of light and heat may be left out of consideration because the phenomena to be accounted for are very nearly the same in form and degree on opposite hemispherical surfaces of the earth, and therefore cannot be due to vibrations of that order. There remains the class of æthereal vibrations to which, according to the theory of physics I am advocating, the force of *gravity* is due. These are supposed to be of such an order that they are capable of transmission through masses without undergoing sensible diminution or retardation; so that they would act nearly equally on the two halves of the earth's atmosphere turned from and towards the moon. Now the gradation of density of the atmosphere which these vibrations, acting solely as the force of gravity, would produce, will be found by calculation to be far too small to give rise to magnetic streams capable of accounting for the lunar-diurnal variations. It would seem, therefore, that the generation of such streams can only be ascribed to the *propa-*

gation of the gravity-vibrations of the moon through the different gradations of density of the earth's atmosphere. These circumstances are exactly conformable to the conditions which, according to our theory, are required for the generation of magnetic currents, and an adequate effect might be expected to be thereby produced, when the magnitude of the vibrations and the extent of the gradation of the atmosphere's density are considered.

146. Assuming the above account of the generation of terrestrial magnetic currents by the moon to be *experimentally* verified, we might legitimately infer that solar magnetic currents are similarly produced by the propagation of the gravity-vibrations of the planets in the Sun's atmosphere. Also the view that these currents are concerned in causing the outbreaks and varying aspects of the *solar spots* would receive confirmation. (See Arts. 137 and 141.)

147. Supposing, for the sake of simplicity, the moon to be in the plane of the earth's Equator, and the earth's atmosphere to be exactly symmetrical with respect to this plane, the æthereal forces tending to generate magnetic streams would act in meridian planes, and would be exactly equal and opposite on the opposite sides of the Equator, so that no motion would ensue. As, however, that equilibration of the forces does not actually take place, if for no other reason, because of the moon's motion in Right Ascension and Declination, there will be a residual action whereby currents will be produced and maintained, which will be nearly steady and circulating, and at the same time will continually shift their positions with the moon. I shall not here attempt to deduce by à priori reasoning the forms which the lines of motion would take under these circumstances, but it may be worth while to state what indications relating thereto are given by the results of observation.

148. From the data in a Table in page 111 of Walker's *Essay on Terrestrial and Cosmical Magnetism* it appears that the observed mean lunar-diurnal variation would result from magnetic currents flowing *eastward* in *north* latitudes, and *westward* in *south* latitudes on the superficies of the earth turned towards

the Moon, and *westward* in *north* latitudes and *eastward* in *south* latitudes on the opposite face. As the observations shew that the maximum deviations of the needle occur at syzygies and quadratures, the mean positions being at octants, it may be assumed that the velocity of the currents is greatest at syzygies and zero at quadratures. Also as the observations further indicate that the motion vanishes along the equator, and increases within certain limits on each side of the equator as the latitude increases, we may infer that circulating movements take place about the point of the equator to which the Moon is vertical and the opposite point. Or we may regard the total motion as consisting of two inclined systems of currents circulating round the earth in opposite directions and intersecting at quadratures. It is interesting to notice the analogy of these laws to the distribution of the spots on the Sun's surface, the number of which is observed to be at a minimum along the equator, and to increase with the distance from the equator on each side within zones of certain breadths.

149. The preceding arguments and explanations may suffice for my present purpose, which is only to give *primâ facie* evidence that the proposed theory of the generation of magnetic currents is capable of embracing the intricate facts and laws of Terrestrial Magnetism without having recourse to any other principles than those on which the general theory of the physical forces is founded.

150. It is proper to state here that with the exception of the æthereal currents generated by frictional electricity (see Arts. 88 and 89), all that have hitherto been under consideration, however they may have been produced, have been called *magnetic* currents, for the sake of distinguishing them from *galvanic* currents, the specific character of which I proceed now to discuss on the principles of the general hydrodynamical theory.

151. GALVANISM. The fact that galvanic action may be transmitted to an unlimited distance along a fine cylindrical wire gives the means of deducing at once a hydrodynamical theory of galvanism. For according to Hydrodynamics it is a necessary condition of the steady flowing of a stream of the æther in a

constant direction, that together with the movement of the fluid in that direction there should be transverse circular motion about an *axis*[1]. The axis of the cylindrical wire furnishes an axis of symmetry for the motion of the fluid: but together with symmetrical motion, both within and outside the wire, parallel to this axis, there must be transverse circular motion about it; for otherwise, since by the contraction of channel the velocity is greater, and the pressure less, within the wire than in the contiguous space, the fluid would flow from all sides towards the axis, and thus a stop would be put to the current. The *spiral* motion which results from the composition of the longitudinal and transverse motions, being accompanied by centrifugal force due to the circular motion, has the effect of maintaining the current. This spiral motion is the distinctive characteristic of a *galvanic* current.

152. When the continuity of the wire, or of any substance by which a galvanic current is conducted, is abruptly broken, at the first instant the stream issues from the terminal by reason of its momentum, and impinges on the surrounding fluid. But since it ceases at the same instant to be maintained by circular motion about an axis, the compound motion is immediately converted into the kind which for distinction I have called *magnetic*, and consequently, in accordance with a general law of such motion, is turned back by having to encounter the inertia of an unlimited mass of the fluid. The return course will be along the original conductor if this be the only, or the readiest conductor. If, however, another wire conductor should be in the neighbourhood of the first, there would seem to be no reason why the revulsion due to the fluid's inertia should not cause a partial return of the fluid along this wire also. In fact, it has been proved experimentally by Faraday that such motion actually takes

[1] This proposition is discussed in the *Principles of Mathematics and Physics*, pp. 563—570; but it is more completely demonstrated in Arts. 16—30 of a communication on the "Hydrodynamical Theory of Magnetism" in the *Philosophical Magazine* for June, 1872. The analytical condition of the spiral motion is that $udx + vdy + wdz$ be integrable by a *factor*.

place. This then is the explanation, according to the proposed hydrodynamical theory, of the *induction* of a galvanic current; whereby a large class of phenomena exhibited in galvanic experiments may be accounted for.

153. The inquiry as to the *origination* of the rectilinear and circular motions which combine to form a galvanic current is distinct from the preceding investigation, inasmuch as it involves, in addition to inferences from hydrodynamical principles, considerations respecting chemical action between dissimilar bodies, as also respecting the effect of the particular arrangement of the atoms or molecules of the conducting substance. On these points it is requisite now to make some remarks.

154. Galvanic currents may be conceived to be generated in the same manner as magnetic, so far as regards the condition of a gradation of atom-density, the gradation being in their case produced and maintained by chemical action in such manner as to exist permanently in the neighbourhood of the surfaces of contact of the substances between which the action takes place. The currents thus generated must, from what has been already argued, fulfil the condition of flowing in a complete circuit in order that they may be permanent; for which reason it is necesary to connect the poles of a galvanic battery by some material, as copper wire, capable, in respect both to form and quality, of conducting galvanic currents. But from the foregoing mathematical argument it appears that currents so conducted can proceed only in *spiral* courses. Now, assuming the course to be of this kind, it follows from experimental indications that the *turn* of the spiral is always in the *same* direction relative to the course of the current along the conductor. Hence it would seem that the spiral character of the course is impressed by the conducting substance itself, because as far as regards hydrodynamical conditions the course might either be *dextrorsum*, that is, to a person looking along the axis in the direction of the current, from the left hand to the right, *above* the axis; or *sinistrorsum*, that is, under the same circumstances, from the right hand to the left.

155. Assuming, therefore, that after the generation of the

currents by chemical action, the spiral course and direction of the turn of the spiral are determined by the atomic or molecular constitution of the conductor, it is conceivable that these effects may be due to a particular *arrangement* of the constituent atoms, whereby the path of least resistance, instead of being rectilinear, is continuously made to assume a spiral form. For instance, such a modification of the path might be produced if the mean retardation due both to the reaction and the arrangement of the atoms, operated in a direction not exactly opposed to that of the stream, but inclined thereto in a constant manner. The capacity for thus producing spiral motion, and, in consequence, conducting galvanic currents, which exists in different degrees in different substances, seems to be eminently possessed by *copper*, on which account wires used as *rheophores*, or conductors of galvanic currents, are generally of this metal.

156. It is not possible to determine by theory alone either the direction of a galvanic current, or whether the spiral motion be dextrorsum or sinistrorsum. But from experiment it may be inferred that the direction of the current and that of the turn of the spiral are related to each other in a constant manner, and by combining theory with experiment the law of the relation may be deduced as follows. Reasons have already been given for supposing magnetic currents to issue from a horizontal needle at the *south* or *positive* pole, and to enter at the *north* or *negative* pole. Let us, therefore, on the ground of analogy adopt the hypothesis that currents always flow, or tend to flow, *out of* poles which experimenters call *positive*, and *into* those which they call *negative*. Now the pole of a voltaic pile, or galvanic battery, which has the *zinc* terminal plate is called *positive*, and that which has the *copper* terminal plate *negative*. Hence, by the above hypothesis, the current along the connecting rheophore outside the pile or battery flows *from the zinc to the copper*.

157. Supposing the direction of a galvanic current to be determined by this condition, it is found by experiment that the coexistence of the southward-flowing horizontal component of the terrestrial magnetic current with a galvanic current along a

moveable vertical rheophore, produces an action on the rheophore tending *westward* or *eastward* according as the current is *ascending* or *descending*[1]. These facts are explainable by the theory on the supposition that the spiral motion is *dextrorsum*. For in that case the magnetic current and the circular part of an *ascending* current are opposed to each other on the *east* side of the wire, and concur on the *west* side, and consequently, by the hydrodynamics of steady motion, the result is an excess of pressure on the individual atoms of the rheophore *westward*. Similarly, the magnetic current and the circular part of a *descending* galvanic current are opposed to each other on the *west* side of the wire and concur on the *east* side, and the consequent action is *eastward*. This argument decides that the spiral motion is dextrorsum on the hypothesis that the galvanic current flows from the zinc to the copper along the connecting rheophore. In like manner it might be shewn that the spiral motion is sinistrorsum if the current be from the copper to the zinc.

158. Since theoretically these two courses are equally possible, the actual course is determined by the particular circumstances of the battery and the rheophore, and can only be known by the aid of experiment. It is not easy to fix upon experiments proper for this purpose; but the galvanic arrangements employed for transmitting *telegraphic messages*, when regarded from a theoretical point of view, will, I think, enable us to obtain the required information. According to those arrangements the *positive* pole of the battery at the place from which the message is sent is connected by a rheophore of fine wire with the place at which the message is received, and at this position the end of the wire is brought into close connection with earth-matter by being sunk into the ground. The effect of this junction between the wire and the earth appears to be intensified by placing the end of the wire in contact with a vertical metallic plate sunk to a considerable depth below the surface. The *negative* pole of the battery is in like manner connected with the ground; but *no circuit is formed.*

[1] The experiments referred to are described in Arts. 732 and 734 of Atkinson's Edition of *Ganot's Elements of Physics* (1866).

159. Now, according to Hydrodynamics, it is not possible that an æthereal current, issuing out of the earth at one end of the rheophore and entering into it at the other, can continue to flow. The inertia of the general mass of the æther would be opposed to both the issuing and the entering of the stream, and, supposing it to be generated at any instant, would quickly cause it to cease. Consequently we must suppose that under the above-stated conditions of earth-connection, *two* streams are simultaneously generated, and that they flow in opposite directions. Of the stream which flows in the direction from the *negative* to the *positive* pole of the battery, the part between the positions of despatch and receipt may be supposed to be generated by a push or impulse in that direction, and therefore to be a *condensation*-stream, that is, one whose density somewhat exceeds that of the fluid in its quiescent state. The part in the opposite direction between the battery and the ground will consequently be a *rarefaction*-stream, as being generated by a *dragging* of the fluid, and will flow in the same direction as the other. The contrary stream, simultaneously generated, and flowing in the direction from the *positive* to the *negative* pole, must have the same velocity as the other, in order to satisfy the above-stated hydrodynamical condition, and must at each point be just as much condensed or rarefied as the former is rarefied or condensed. Thus, since by Hydrodynamics the two streams can coexist, there will neither be residual motion parallel to the axis of the wire, nor residual condensation or rarefaction.

160. But on taking account of the circular motions belonging to the two streams, it will be seen, since the streams are generated by the same chemical action and flow in opposite directions, that if the circular motion in one be dextrorsum in the other it will be sinistrorsum (see Art. 157). Now as the currents flow in opposite directions, the dextrorsum circular motion of the one will coincide with the sinistrorsum circular motion of the other; so that the residual motion will be exclusively circular motion about the axis. The circular motion of the stream produced by *impulse*, and that of the stream produced by *dragging*, as described above, will be propagated in the *same* direction, because, according to Hydro-

dynamics, a condensing disturbance is propagated in the direction of the impulse, and a rarefying disturbance in the direction contrary to that of the dragging. Hence the two circular motions will coalesce and be propagated together. The direction of the compound motion will be *dextrorsum* between the two stations, assuming it to be determined by that of the condensed issuing stream, which, as corresponding to the stream of a *closed* circuit flowing externally from the zinc to the copper, will, according to the principle adopted in Art. 156, be dextrorsum[1].

161. There will be a like propagation of transverse circular motion in the opposite direction from the battery, which with respect to that direction will be dextrorsum, but relatively to the propagation in the first direction, sinistrorsum. Both sets of circular motions will be dispersed without revulsion by entrance into the ground, this effect being probably accelerated by the connection of the wire with the metallic plates. Very little of the force of the battery is expended in transmitting the transverse circular motion which enters the ground in the negative direction, because of the short distance it has to travel. The force is chiefly required for generating a sufficient amount of such motion for transmission from the despatch station to the receipt station notwithstanding its continual diminution by the *resistance* it encounters from the atoms of the conductor.

What has now been said may suffice for indicating the way in which the hydrodynamical theory of galvanism accounts for the conditions under which telegraphic messages are despatched by means of galvanic action. I proceed next to the consideration of *Oersted's Experiment.*

162. Assuming the movements of magnetic and galvanic currents to be such as the foregoing discussion indicates, let a galvanic current be made to pass *from north to south* along a conducting-wire placed *over* a magnetic needle in the magnetic

[1] This theoretical explanation of the effect of the double connection with the earth for telegraph despatches, and of the necessity for such connection, has only recently occurred to me, and is given with reserve. It seems to indicate in what respects the motion in a closed circuit differs from that maintained by earth-connection.

meridian, as in one case of Oersted's experiment. Then supposing the converging streams of the magnet at the *north* end, and the circular part of the galvanic current, to be resolved in a horizontal plane passing through the axis of the needle and perpendicularly to that axis, the two resolved portions will concur on the *west* side of the needle and be opposed to each other on the *east* side, so that the needle will be urged *westward*. Similarly at the *south* end the circular part of the galvanic stream and the resolved parts of the divergent magnet streams concur in direction on the *east* side and are in opposite directions on the *west* side, and that end will thus be urged *eastward*. Hence on both accounts the *north* end deviates towards the *west*. The above circumstances agree with those exhibited by Fig. 64 in page 209 of the Astronomer Royal's Treatise *On Magnetism*, and the theoretical result is verified by the deviation of the needle there given as known from observation.

The other cases of Oersted's experiment exhibited in pages 209 and 210 of the same Treatise may be similarly explained by the hydrodynamical theory. The reasoning in each case is so like the foregoing that the insertion of it here may be omitted.

163. One of the most remarkable phenomena relating to magnetism is the effect which a mass of copper in the neighbourhood of a magnetic needle has upon its vibrations, the extent of which it tends continually to diminish till the needle is brought to rest. The explanation of this fact by the hydrodynamical theories of magnetism and galvanism rests on a simple magneto-galvanic law established experimentally by Faraday. When a plate of copper $1\frac{1}{2}$ inch wide, $\frac{1}{5}$ of an inch thick, and 12 inches long, was placed with its faces at right angles to the line of junction of the poles of a powerful horse-shoe magnet, and the terminals of the wire of a galvanometer were put in contact with the long edges, it was found that a galvanic current was developed as soon as the plate was caused to move transversely to the magnetic current (*Phil. Trans.* 1832, p. 151). Now, by hydrodynamics, the displacement of the æther by the finite spherical atoms of the copper in motion would give rise to a flow of æther, the mean direction of which *within the plate* would be the same as that of the impulse,

whilst the mean flow of the return streams outside the plate, by which the *circulation* is completed, would be in the opposite direction. Hence supposing, for the sake of definiteness, the plate to be held with its faces horizontal and the long edges parallel to the meridian of the place, on moving it northward or southward a current would be produced in the same direction which would coexist in the plate with the above-mentioned magnetic stream. The galvanometer indicated that under these circumstances a *galvanic* current was generated which flowed from one of the long edges of the plate and after completing the circuit entered at the other. It follows from this fact, taken in conjunction with our theory of galvanic currents, that the motions, in rectangular directions, of the two above-mentioned currents produce, by the intervention of the atomic constitution of the copper, *circular* motions parallel to the meridian, together with a current parallel to the axis of the galvanometer's wire, and that thus the galvanic stream is constituted. This is, in fact, equivalent to the ordinary mode of producing a galvanic circuit, the combined action of the galvanic and magnetic currents taking the place of chemical action. It must accordingly be supposed that the combined currents in producing this effect overcome a certain amount of *resistance*, just as the chemical action does. The resistance arises from the reaction of the atoms, which requires the circular motion to be continually maintained by an impressed force.

164. The direction in which this galvanic current flowed may be inferred (on the hypothesis already adopted that copper possesses the property of giving to a current a dextrorsum spiral course) from the following additional circumstances of the experiment. The wire proceeding from the east side of the plate was made to pass from north to south *beneath* a horizontal needle in the magnetic meridian, and the north end of the needle was observed to "pass eastward" when the plate was moved northward. Hence if we assume that the current flowed from the *east* side of the plate, the circular motion about the rheophore would conspire on the *east* side of the *north* end of the needle with the

entering magnetic streams, and be opposed to them on the *west* side. The north end of the needle would consequently be urged *eastward*, in accordance with the experimental result stated by Faraday. The assumed direction of the current is thus confirmed; and it may be further remarked that as the circular motion resulting from the composition of the two streams above mentioned might either be dextrorsum or sinistrorsum, the fact of its being always dextrorsum is attributable exclusively to the atomic constitution of the copper. When the plate was moved *southward* the north end of the needle passed *westward*, shewing that the galvanic current, assumed to be still dextrorsum, flowed out of the *west* side of the plate.

165. The effect is the same whether the plate be moved, the magnet being fixed, or the magnet be equally moved in the opposite direction, the plate being fixed. For since the magnet and its streams have a *fixed mutual relation*, if we conceive the magnet's motion to be impressed both upon it and upon the plate in the opposite direction, the magnet and its streams will be in the same case as when the magnet is at rest, and there will only remain the effect of the motion of the plate in the direction opposite to that of the magnet's original motion. Thus opposite equal motions of the magnet and the plate have the same galvanic effects. In all cases the effect is proportional to the *relative* motion between the magnet and the copper.

166. Although in the usual series of experiments which exhibit the mutual action between a magnet and a mass of copper relatively in motion, the generation of galvanic streams in the latter is not ascertained by a galvanometer, we may yet infer from the experiments described above that in all cases there exists the tendency to produce a circulating galvanic current, and that thereby a *resistance* to the magnetic currents is generated (see Art. 163). Since, on the other hand, the magnetic currents tend always to maintain the same velocity and form, and *the same relation to the magnet*, the general result will be, a diminution of the resistance by a resulting accelerative force acting so as continually to diminish the relative motion on which the resist-

ance depends. Accordingly, without investigating particularly the modus agendi, we may conclude that in all cases there is a gradual decrement of the relative motion between the magnet and the copper[1].

167. On this principle the decrement of the excursion of a needle vibrating in the magnetic meridian in the neighbourhood of a mass of copper is at once accounted for. Supposing the copper to be stationary, the action on the needle is an accelerative force always opposed to its motion, and greater as the motion is greater; operating, therefore, like a resisting medium in bringing it to rest.

168. The same principle explains the dragging of a horizontal needle by a rapidly rotating circular plate of copper, or the dragging of a circular plate of copper by a large rotating magnet, the operation taking place in such manner that the relative motion continually diminishes and eventually disappears.

169. If the magnet be a horizontal needle, a uniformly rotating circular plate of copper might, by acting upon it so as just to counteract the force of terrestrial magnetism, keep it in equilibrium out of the magnetic meridian.

170. It is not necessary for my purpose to discuss at greater length the explanations of the foregoing class of phenomena. With respect to the theories of *the mutual action between galvanic currents,* of the action of *rheophores of the form of a helix,* and of *Ampère's Solenoids* (which are treated of in pages 594—599 of the *Principles of Mathematics and Physics*), I shall only remark here that the facility with which they furnish explanations of the respective phenomena confirms very much the conclusion to which the whole antecedent theory of galvanism points, namely, that *galvanic phenomena are governed by hydrodynamical laws.*

171. The *magnetization* of iron by a cylindrical coil of copper through which a galvanic current is sent admits of the following theoretical explanation. Let the current, always assumed to flow

[1] Respecting the general principle here employed see the second paragraph in page 643 of the *Principles of Mathematics and Physics*. Various cases of experiments of this class are discussed in pages 636—644 of that work, and in Arts. 35—40 of the communication to the *Philosophical Magazine* for June, 1872.

in the rheophore from the zinc to the copper terminal plate of the battery, receive from the coil a *dextrorsum* spiral course, and suppose the turns of the coil to be very close together. Then the streams along and parallel to the axis of the wire will be moving in opposite directions in space at the ends of any diameter of the coil, and consequently along the diameter and its prolongations they will to a certain amount neutralize each other, but in greater degree within the coil than outside. On the other hand, the *circular* motions about opposite portions of the coil will *conspire,* especially in the vicinity of its axis, along which they will form by their composition a stream, which will enter within the coil by converging courses at one end, and issue from it by diverging courses at the other. As both the coil and the galvanic current are supposed to be *dextrorsum,* it follows that the entrance takes place at the end at which the galvanic current enters. This stream is evidently like in form to that of a bar-magnet, and, in fact, it appears from experiment that when such a coil, or solenoid, is suspended so as to be free to move horizontally, the issuing end is caused to point *southward* by the directive action of terrestrial magnetism, just as in the case of a suspended bar-magnet[1].

172. Now let a cylinder of soft iron be placed within the coil so that its axis and that of the coil are coincident. Then the above-described composite stream would be precisely of the form proper for magnetizing the iron, especially as regards the terminal converging and diverging courses (see Art. 116). Experiment shews that soft iron is instantly magnetized by this process, and also, as might be inferred from the theory, that the magnetism disappears as soon as the current is stopped. If a cylinder of

[1] The Fig. 604 in Tom. III., p. 253, of Jamin's *Course of Physics,* indicates that the current in the coil is *from the copper to the zinc* plate of the battery, and that the turns of the coil are *sinistrorsum.* Since according to the present theory the spiral course of the current would in that case also be sinistrorsum, the composite stream would, as before, enter and issue at the same ends as the galvanic current. To prevent misapprehension I take this opportunity to state that in Ampère's Theory the terms *dextrorsum* and *sinistrorsum* are applicable only to the turns of the *coil,* there being nothing in that theory corresponding to spiral galvanic streams.

steel be put in the place of the soft iron, it is found that it receives by degrees a large amount of magnetism, and may thus be magnetized to *saturation.*

173. The foregoing theory of the induction of magnetism by a galvanic current, combined with the theory of the induction of a galvanic current given in Art. 152, will account for the construction and action of Ruhmkorff's apparatus.

174. In the case of a galvanic coil *surrounded* by a cylinder of soft iron having the same axis, and of moderate thickness, the circular motions about opposite portions of the coil will neutralize each other where they coexist within the iron; also there will not be the same coalescence of the circular motions as that in the neighbourhood of the axis in the case of the cylinder within the coil, and the convergency and divergency of the lines of motion, on which the magnetizing action mainly depends (see Arts. 114 and 116), can only be of small amount. For these reasons it seems allowable to say that the iron cylinder would give little or no indication of induced magnetism. Unhappily I am not able to cite any experiment by which the truth of this theoretical deduction might be tested. I have, however, thought that the argument might properly be stated here, because Ampère's theory of magnetism does not lead to the same result[1]. Although the theory of galvanism I have proposed explains satisfactorily all the *facts* relating to Ampère's solenoids, there was no reason to expect that it would accord with mathematical deductions from his *theory of magnetism,* inasmuch as, according to hydrodynamical principles, galvanic currents differ, on account of their spiral courses, essentially from magnetic currents, and could not possibly account for the existence and laws of the latter.

175. To complete the theory of galvanic currents a few words are required to be said of those which are usually called *thermo-electric,* which, according to the views previously explained, should rather be called *thermo-galvanic.* The existence of these currents is

[1] This, at least, I gather from an *Investigation of the Attraction of a Galvanic Coil on a small magnetic mass* by Mr James Stuart, published in the *Proceedings of the Royal Society* (No. 140, p. 66).

particularly corroborative of a theory which makes the generation of currents in any substance depend on gradations of density in its interior. If a homogeneous conducting wire of a galvanometer be heated at any point sufficiently distant from its extremities, no current is perceptible, although the heat must produce gradation of density. The explanation is that under these circumstances there is no reason for the flow of a current in one direction rather than the other, so that any currents that might be produced would be equal and opposite, and would neutralize each other. But if the wire be dissimilar on the two sides of the point of heating, by reason of knots and contortions made on one side, a *galvanic* current is obtained, not simply because of the dissimilarity, but because the contortions produce displacements of the relative positions of the atoms, whereby, in conjunction with the gradation of density due to the heat, the spiral motions are produced which are necessary to constitute a galvanic current. This, accordingly, is an instance in which, as in Faraday's experiment, a galvanic current is generated without chemical action (see Art. 163). It has also been found by experiment that galvanic currents are connected with particular circumstances of *crystallization;* which, again, is evidence that the circular motions are dependent on particular arrangements of the atoms.

176. In Seebeck's experiment with two metals soldered together, the circular motion may be conceived to be generated by unequal displacements of the relative positions of the atoms of the two substances at the heated place of juncture.

The following supplementary remarks and explanations are appended for the purpose of indicating more particularly the characteristics and limitations of the general theory of the physical forces discussed in the foregoing Essay. I propose, first, to state for what reasons the names I have employed in speaking of these forces have been selected, and to shew why I could not consistently with the principles of the theory accept certain terms and designations which appear to have had their origin in the conceptions

of experimentalists respecting the nature and relations of the different forces. This discussion will serve to indicate in what respects especially the hydrodynamical theory is distinguished from views admitted by physicists of the present day. I shall next endeavour to point out how far the theory has been satisfactorily supported by mathematical reasoning, in what respects it is imperfect, and by what course it may be expected to be made more complete. In conclusion, I propose to draw from the theory as a whole, on the presumption that its truth has been sufficiently established by the antecedent arguments, some inferences respecting the quality of physical *causation.*

The *nomenclature* which I have employed in speaking of the different kinds of force was adopted for reasons which, as they could not well be given till the theories of the respective forces had been exhibited, may be properly stated here. Throughout the Essay I have used exclusively the original terms, *Electricity, Galvanism,* or *Voltaism,* and *Magnetism,* not alone because of their *historical* appropriateness, but because they distinguish between the respective physical forces exactly in accordance with the *à priori* views which it was the purpose of the Essay to establish. For this reason I have not had occasion to use such terms as "statical electricity," "dynamical electricity," "electro-magnetism," and "thermo-electric." These names are not simply significant of matters of fact, inasmuch as they involve *speculative* inferences drawn by experimentalists from the results of their experiments. They may, however, be regarded as means of classifying facts, and indicating mutual relations of different classes of facts; but in order to determine precisely their physical signification, a *theory* of the physical forces founded on *necessary* hypotheses, and supported by mathematical reasoning, is required. As the general hydrodynamical theory maintained in the Essay is professedly of this character, I propose now to make use of it for ascertaining the meaning and applicability of the above-mentioned speculative terms.

According to prevailing usage, the effects produced by an electric machine, and those resulting from the action of a voltaic pile, or galvanic battery, are called by the same name, *Electricity.*

This has been done, first, because the poles of a voltaic pile exhibit some amount of electric tension, and again because, as Faraday shewed, a current of frictional electricity has in a slight degree the power of decomposing water like a galvanic current. But to produce by this means a very small amount of decomposition requires a quantity of electricity so "enormous" that Faraday says he "was almost afraid to mention it." He calculates that 800,000 charges of a Leyden battery, charged each time by 30 turns of a very large and powerful plate electric machine, would be necessary to supply electricity sufficient to decompose a single grain of water! (*Researches in Electricity*, Nos. 860 and 861.) These facts prove that although frictional electricity and galvanism may, under special circumstances, give rise in some degree to phenomena of the same kind, they are in essential respects widely different. What they have incidentally in common may be ascribed to their being modes of action of the *same* medium, the æther. In drawing from the above-stated facts the inference that the two forces are *identical*, Faraday seems to have taken no account of other facts which more decidedly indicate non-identity; for instance, if a metallic wire connected with an insulated conductor charged with frictional electricity be touched with the finger, the electricity is immediately dissipated, whereas the action of a galvanic battery is not sensibly disturbed by touching in the same manner a wire which completes the circuit. In short, the quality of that which the proof-plane detects is very different from the quality of the current indicated by the galvanometer.

According to the hydrodynamical theories of electricity and galvanism explained in the Essay, it appears that the electric state of a substance wholly depends on abnormal relative positions of the atoms in a very thin stratum contiguous throughout to its surface, and that in connection with this condition there exists a gradation of interior atom-density whereby steady streams are generated (Arts. 74—78). But these streams, although they account for electric attractions and repulsions (Arts. 92 and 93), do not, any more than those of a magnet, possess the spiral quality of galvanic streams. This explains how it was that Faraday was able

to generate by an enormous expenditure of electric force only a very small amount of galvanic action; so small, in fact, that possibly it might have originated in the conducting-wire. Also it may be said, consistently with the indications of the hydrodynamical theory, that galvanic spiral streams may co-exist with a small amount of superficial electric tension, especially at the terminals of a battery when the circuit is very nearly closed. Thus the facts from which the identity of electricity and galvanism was inferred are explainable by our theory without recognition of such identity, but rather in contradiction to it, inasmuch as the explanations depend upon a condition which specially distinguishes electricity from galvanism, namely, the existence of an abnormal state of the atoms in a thin superficial stratum of the electrified body.

A distinction is actually admitted in the use that is made of the terms "Electrostatics" and "Electrodynamics," the former of which expresses what I call Electricity, and the other includes what I call Galvanism. The common prefix "Electro" shews, however, that those who originated these terms comprehended galvanism under the name of electricity. From the point of view of my theory I am unable to perceive the applicability of the adjuncts "statics" and "dynamics," the distinction between electricity and galvanism having no analogy to that between the state of rest and the state of motion in ordinary Mechanics. There are electric as well as galvanic currents which produce attractions and repulsions, and at the same time the currents in the two classes of phenomena are, according to what is said in the preceding paragraph, of different kinds.

The term "Electrodynamics," as usually employed, is not, however, restricted to galvanic phenomena, but is taken to include those resulting from the co-existence and mutual action of galvanic and *magnetic* streams. Although according to the theory I maintain, the two classes of phenomena are referable to modes of action of the *same* medium, and might on theoretical grounds be put under the same denomination, I cannot but think that for the sake of precision in the statement of experiments, a name should

be given to the latter class expressive of its relation to magnetism. They might, for instance, be regarded as phenomena of magnetism modified or produced by galvanism, and accordingly be designated as *Galvano-magnetic.*

Phenomena produced by means of galvanic currents in metallic coils, so disposed as to give rise to attractions and repulsions obeying apparently the same laws as those of a natural magnet, have been ranged under the term "Electromagnetism." The part "electro" of this term was adopted from the supposed identity between electricity and galvanism, and "magnetism" was added in reliance upon Ampère's theory of the identity of magnetism with the action of galvanic currents in solenoids. Now, as I have maintained in my work on the *Principles of Mathematics and Physics* (pp. 594—598), the facts relating to the action of solenoids, so far as they have been ascertained by *experiment,* are readily explainable by the hydrodynamical theories of galvanism and magnetism; whereas *calculation according to Ampère's theory* gives an amount of magnetism induced in a thin cylinder of soft iron surrounding symmetrically a galvanic coil, in disagreement (as was mentioned in Art. 174) with the amount deducible from the above-named theories, being much larger. I am unable at present to appeal to experiment for deciding between the two results; but it is, at least, evident that the theories cannot both be true, and that if one can be proved to be true the other must be abandoned. In short, the hydrodynamical theory of galvanic currents is wholly irreconcileable with Ampère's theory of magnetism, these currents being incapable, by reason of their spiral motion, of forming magnetic currents.

According to these views the phenomena which have been called "electromagnetic," are either simply *galvanic,* as partaking neither of electricity proper nor of magnetism proper, or come under the denomination, *galvano-magnetic,* proposed above.

To complete this discussion of the nomenclature which is in accordance exclusively with the hydrodynamical theory of the physical forces, it is farther to be remarked that currents which act upon the needle of a galvanometer are producible in *three* ways; by

chemical action, by a magnetic current (Art. 163), and by heat. Accordingly for distinction the three kinds of currents may be called respectively, *chemico-galvanic*, *magneto-galvanic*, and *thermo-galvanic*.

It is, I think, to be regretted that experimentalists use the word "Electricity" with so great latitude as to its applications. The practice seems to have arisen from speculatively inferring the identity of different physical forces from phenomena which they have in common, and not giving sufficient consideration to other circumstances by which they are distinguished. The hydrodynamical theory accounts for different forces having incidentally common qualities by their being modes of action of the *same* medium. The indiscriminate application of the same term (electricity) to things as widely different as a charge by an electric machine, and a galvanic current, must have the effect of producing confused notions respecting the physical signification of the term. I take this opportunity to state that in my physical researches I have uniformly regarded electricity as an effect produced by friction, or other means, exclusively at the *surfaces* of substances. I have noticed with satisfaction that the Astronomer Royal employs in the Greenwich Observations the terms "galvanism" and "galvanic signals;" but "*Electric* Telegraph" is, I fear, too established by common usage to be unsettled.

I propose now to state, as well as I may be able, how far the researches detailed in the Essay relative to the character and laws of the physical forces, have been successfully carried on, and what course remains to be pursued by those who may be disposed to continue the investigations on the same principles.

With respect to the Undulatory Theory of Light, I conceive that all explanations of properties of light which depend solely on mathematical reasoning applied to the æther according to hydrodynamical principles, have been effected in such manner as to leave little to be desired (see Art. 37). These explanations are specially adapted to prove the reality of the hypothetical æther. Upon farther consideration of the Problem of Diffraction I have seen reason to conclude that the method of solution adopted by Fresnel is a legitimate inference from the principle

that the undulations are composed of *primary* vibrations in the manner described in Art. 41, and that consequently that method is hydrodynamically true.

In Arts. 13—15 of a communication to the *Philosophical Magazine* for June, 1872, on the Hydrodynamical Theory of Magnetism, I have discussed to some extent a new and interesting inference from Hydrodynamics, namely, that impulses which are not vibratory may give rise to vibratory motions of an elastic fluid. The impulses may either be given equally in all directions from a centre, or by a spherical solid moving in the fluid. The vibratory motion thus produced is due to the inertia of the fluid, and the method of calculating it depends (as in the Problem of Diffraction) on the laws of the primary vibrations, which, as having been investigated apart from any extraneous action, result exclusively from the elasticity and inertia of the fluid (see Art. 53). This question, which has an important bearing on the theory of the origination of rays of light, deserves more consideration than that I have given to it in the above-cited communication.

The hydrodynamical theory of light accounts satisfactorily for the modes of generation of polarized rays, and for the laws of polarized rays of different kinds; and with respect to polarization by Double Refraction and Reflection it has given explanations on some points left undetermined by Fresnel's researches. (See Art. 46, and the discussions under *Problems* II. and III., in pp. 375—415 of the *Principles of Mathematics and Physics.*) Much, however, remains to be done relatively to the whole class of phenomena depending upon the Reflection and Refraction of light at the surfaces of crystallized and non-crystallized media. The Undulatory Theory of Light has not yet been applied in a general manner on hydrodynamical principles to these questions, and probably to do so effectively it would be necessary to obtain exact expressions for the laws of atomic repulsion and molecular attraction, as resulting from a certain hypothetical *arrangement* of the atoms of the substance. In short, the complete solution of this class of problems might be expected to involve mathematical theories of Chemistry and Mineralogy.

The general hydrodynamical theory of attractive and repulsive forces (see Arts. 49—56), which is applied in the theories of atomic repulsion, molecular attraction, and gravity, is admitted to be incomplete, although it depends solely on the solution of a problem which presents no other than mathematical difficulties. The problem is to determine whether a small sphere acted upon by a series of ætherial undulations, is caused not only to vibrate, but to move also in a given direction with an accelerated velocity. The theoretical reasoning employed went so far as to assign a *vera causa* for such acceleration, and to shew that it might under different circumstances be either towards or from the source of the undulations; so that, in fact, it accounted for attractive and repulsive forces. But as no quantitative expressions for the forces containing explicitly the constants of the problem were obtained, the solution was left imperfect (see Art. 67). The difficulty was to calculate precisely the lateral action which takes place in consequence of the break of continuity of the front of the wave by the reaction of the atom. I am now of opinion that this calculation might be effected by treating the wave exactly as in the case of the Problem of Diffraction; but I have not attempted to do so, and can at present only recommend any one who may be disposed to carry on the kind of physical research which I have initiated, to undertake this calculation in particular, on account of its great theoretical importance.

In connection with the hydrodynamical theories of heat, and of atomic repulsion and molecular attraction, *the law of the mechanical equivalence of heat*, which has been established by experiment, might have been discussed theoretically. In fact, it follows evidently from the adopted theory of the constitution of masses, that they act upon each other only by the intervention of the immediate action of the atomic and molecular forces on their constituent atoms. Hence, according to the general theory of attractive and repulsive forces, whether the result of a mechanical impulse be an accelerated motion common to all the parts of the impelled body, or relative motions of parts in vibration, or disruption of parts, the translatory accelerations of the individual atoms are always

accompanied, to a certain proportional amount, by vibratory motions of the same, proper for generating heat-undulations in the æther. Thus the mechanical action is productive of a proportionate quantity of heat. This theory, which I have only slightly touched upon in my physical researches, is open to further development. I am not aware that any other *theory of the modus agendi* from which the equivalence results has been proposed.

It is proper to state that I consider some of the views respecting *terrestrial and cosmical magnetism* contained in Arts. 120—149 to be only probable inferences from the general theory, as not admitting in the actual state of science of being sustained by exact mathematical reasoning. I think, however, that the explanations offered give *primâ facie* evidence that the theory is capable of including this class of phenomena without modification of its hypotheses.

In concluding these remarks I beg to direct attention more particularly to the chief characteristics of the general theory, namely, that its hypotheses are few and perfectly intelligible, and that they have been made the basis of the explanation of a great number of phenomena of various kinds, without in any respect being deviated from or supplemented. So remarkably has this been the case that it does not seem possible to account for it except on the supposition that the hypotheses are true. The "consilience" of the explanations (to borrow an expression from Whewell) has been such that it cannot be attributed merely to human ingenuity. Moreover the possibility of stating the principles, the reasoning, and the results of the reasoning, of so comprehensive a theory in the manner exhibited in the foregoing Essay, and with so little aid from the use of symbols, may, I think, be appealed to as additional evidence of its truth.

For these reasons, notwithstanding the admitted imperfection of the theory in its actual state, and the probability that particular details of it will require modification or correction, I venture to say that the whole preceding argument justifies the conclusion that *the right method of theoretical philosophy has been employed.*

It is unnecessary to dwell on the importance of this result

as regards both the prosecution and the teaching of physical science. According to what has been said in Arts. 10 and 26—28, the method confirmed thus by argument is the legitimate sequel of the theoretical philosophy initiated by Newton, and was pointed out by him prospectively as the necessary means of extending the boundaries of Natural Philosophy. It claims, I think, on this account to be, at least, regarded without prejudice by the mathematicians of Newton's own University. I urge this consideration the more from having observed that at the present time modes of philosophizing are pressed on the attention of Cambridge mathematical students which, in my judgment, are incompatible with teaching that form of physical science which, as Newton was the founder of it, might for distinction be called the Newtonian Philosophy. I continue to hold the opinion expressed in p. xlviii. of my work on Mathematics and Physics, that "the principles of philosophy which Newton inaugurated have been widely departed from in recent times both in England and on the Continent." Great as may be the advances that have been made by modern scientific productions in the determination of physical laws by experiment and calculation, I can perceive no evidence in them of a like progress in the purely *theoretical* department of physics, or even of a correct appreciation of what I have designated as Newtonian Philosophy. I venture, therefore, to suggest that by a stricter regard on the part of English mathematicians to the Newtonian rules of philosophizing, and also *to the rules of logic* (see the Introductions to this and the former work), a new school of theoretical philosophy might be formed which would place our country in the same position relative to physical science as that it held in Newton's day.

Having in the Essay and the above supplementary remarks sufficiently exhibited the characteristics of the proposed general theory of physics, and explained, as far as was practicable in a work like the present, the arguments by which it is sustained, it only remains to fulfil the intention expressed in p. 95 of enquiring what inferences may be drawn from it respecting the ultimate quality of *physical causation*. Relatively to this enquiry

it must be borne in mind that the hypotheses of the theory are these only: (1) all visible and tangible substances consist of inert spherical atoms of constant form and magnitude; (2) all physical force is either active pressure of the æther, supposed to be a continuous elastic fluid having the property of pressing always proportionally to its density, or passive resistance of the atoms to such pressure due to their inertia and constancy of form. I am not entitled to say that the truth of these hypotheses has been completely established by comparisons of mathematical deductions from them with observed facts, but only that by such comparisons and consequent explanations of phenomena, strong presumptive evidence of their truth has been obtained. At least the evidence is of such kind and degree as may be expected to suggest the enquiry, What, if these hypotheses are true, is the essential quality of physical causation? To this question I shall now endeavour to give an answer.

Since it has been argued (Art. 24) that the hypotheses of experimental dynamics are deducible from the principles of theoretical physics, it may naturally be asked whether the hypotheses of physics are also referable to ulterior principles. To this question I should decidedly give a *negative* answer. In p. 681 of the former work I have indicated a conceivable method of accounting for the law according to which the pressure of the æther varies proportionally to its density; but, for the following reasons, I now maintain that there was no need for any argument on this point. The natural facts that come under human cognizance may be classed under two heads; those which are *facts* in the strict sense of the word, that is, simply *caused* to exist, and those which are produced by the intervention of operations performed according to *laws*. The latter kind are dependent on the existence of the first, and inasmuch as they are regulated according to number, weight, and measure, are properly subjects of scientific investigation. But respecting the character and qualities of the first kind nothing can be antecedently predicated; they must be accepted as ultimate facts. No one, for instance, would think of trying now-a-days to evolve the existence and qualities of an

atom by reasoning from antecedent conditions. The existence and qualities of the æther are in the same case as the existence and qualities of the atom, as to being ultimate facts, because they are in the same case as to being *necessary foundations of scientific enquiry.* (This assertion may be taken to rest on the presumptive evidence of the truth of the general physical theory.) Accordingly the qualities of the atom and those of the æther are equally out of the province of *à priori* investigation. Yet reasons why they are such as they are may be given *à posteriori.*

The remainder of this argument will be best conducted by admitting unreservedly the doctrine of *final causes,* which may be legitimately made use of as an auxiliary in an enquiry respecting the means by which we arrive at an understanding of the facts and laws of the external creation. We have evidence in our faculties and our consciousness that the world was *made,* and that it was made by a Personal Creator for a purpose, because we as individual persons make things, and have a purpose in making them. In like manner, by being conscious that strength and skill are required for making any thing, we can understand that these qualities were necessary for the creation of the world, and consequently that it might have been created, among other purposes, for that of demonstrating the power and wisdom of the Creator.

Now, giving existence to the æther and the atoms and to their qualities would of itself only be evidence of personal will and the exercise of power; the proof of intelligence comes from *operations* arising out of these entities and qualities. Ordinary observation suffices for acquiring the conviction that the results of natural operations performed according to laws, as, for instance, the movement of the Earth about the Sun by the force of gravity, are means adapted to ends, and give evidence to us of the accomplishment of purposes by prevision and intelligence. But more than this, the operations are performed according to laws the evolution of which is comprehensible by scientific research and calculation, inasmuch as they are *calculable* consequences of hypotheses which, after being verified to the extent already indicated, may be re-

garded as expressing the primal conditions of physical operation. It is only, however, by slow degrees and after prolonged intellectual labour, that human intelligence has in some measure succeeded in deriving the laws from the original conditions; whereas by the Supreme Intelligence all such consequences must have been intuitively seen when the conditions were first imposed. Thus the theoretical study of physics is specially adapted to exalt and give distinctness to our conceptions both of the wisdom and the power of the Creator. On the principle of final causes it may be asserted that, together with many other purposes, this was contemplated in giving to the æther and the atoms their specific qualities. If I thought otherwise, I should not be able to consider as justifiable the devotion of many years of my life to physical researches.

According to these views, operating by intervening laws has exclusive reference to *us*, and is not necessary absolutely. Natural laws have no other fixity than that they derive from the fixed purpose of the originator of them to effect certain purposes relative to the corporeal and spiritual natures of man. It is evident, if the proposed physical theory be true, that the laws are constant only so long as the qualities of the æther and of the atoms, on which they wholly depend, are constant; and as we have concluded that these qualities were made such as they are by an immediate exercise of power, by power similarly exercised they might be changed in any manner, and even annihilated. In fact there have been well-attested occurrences in the world which can be accounted for only as being caused by power thus operating. They have been called *miracles*, wonders, apparently on account of their infrequency; but essentially they are only repetitions of creative acts of the same kind as those whereby the elements of the world were originally called into existence. Of course, according to these principles, the possibility of miracles cannot be disproved by physical science.

Of natural things and qualities that come within our knowledge some are *invariable*, continuing to be such as they were at their origin by the same Will as that which created them. Others

vary in *time and space* according to *laws*, which, if the argument of this Essay be good, are not only matters of calculation, but are also proved by the calculation to be sustained by *Power* operating always by means of the universal æther.

I have entered into the foregoing considerations because, as it seems to me, the philosophy I uphold is a complete refutation of materialistic views. These, as far as I can learn, owe their origin mainly to the assumptions that bodies, separated by distance, can act upon each other without the agency of an intervening medium, and that atoms are essentially indestructible. The first assumption is set aside by deducing the law of gravity from the qualities and agency of an all-pervading æther; the other is met by saying that nothing can be certified respecting the qualities of atoms except by means of theoretical investigation such as that which it has been the purpose of this work to indicate, and that no *à priori* judgment concerning their nature or their origin is possible. It has been asserted by some who would repudiate being supposed to be materialists that no atom of matter can ever be destroyed, although it is evident that if matter be *per se* indestructible it could not have been created, for a power that creates can destroy. In short, since that assertion has no foundation to rest upon in *physics*, it cannot be *metaphysically* true; for Metaphysics depends essentially on the antecedent establishment of the principles of Physics. The *laws* of the universe having been shewn by physical enquiry to be maintained by *Force*, or *Power*, which, since we distinctly understand its quality by our own acts and consciousness, we are justified in calling *Personal Energy*, it would be unphilosophical in the extreme to say that the *material* of the universe is self-existent.

I propose to sum up the antecedent argument by the following quotation from the General Conclusion of my former work (p. 684): "It may, I think, be asserted that the completion of these theories will demonstrate the existence of a vast and wonderful *mechanism*, of which not the least wonderful quality is, its being so constructed that we can understand it. If it be objected that we do not know what the æther is, or what atoms are, it seems

sufficient to answer that by personal experience we can understand with what qualities they are endued as elementary constituents of the system of the universe, and that this is enough for enabling us to comprehend the whole of the mechanism. It would be thought unreasonable if an engineer, after explaining the construction of a steam-engine, should be required to say what fire is, or what water is; he would think that his explanation of the mechanism and working of the engine should satisfy the enquirer if it rested on properties of fire and water that are known by ordinary observation." Just so our knowledge of the mechanism of the earth and heavens ought to be regarded as complete, when the explanation of it has been made to depend exclusively on properties of the constituent elements which are perfectly intelligible to us by common experience and observation.

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